THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. IX.—*Tertiary History of the Grand Cañon District;* by CLARENCE E. DUTTON, Captain of Ordnance, U. S. A., 4to, with folio atlas. U. S. Geological Survey, CLARENCE KING, Director. Washington, 1882.—With plate IV.*

THE features of the Grand Cañon of the Colorado and its varied illustrations of river erosion first began to be appreciated after the descriptions and views given in the Report of the Expedition under Lieutenant J. C. Ives in 1857–58, with which Professor J. S. NEWBERRY was associated as Geologist. Major POWELL'S Reports and the photographs made by the Expedition under his direction added greatly to our knowledge and appreciation of the facts, making accepted truth of what had seemed to many to be almost beyond belief.

Captain DUTTON'S Report, just now published, still further illustrates the features of the region, portraying the magnificence of its various parts by remarkably effective views and word-pictures, besides describing its geological structure and history. In the course of the latter the author takes the reader on several excursions through the Grand Cañon among the castellated and cathedral-like peaks and ridges two to five thousands of feet in height which stand over its bottom and project from its deeply alcoved borders; and gives explanations along the way with regard to the peculiar features of the different rocks, the water-sculpturing process, the long and profound faults of the region, the many volcanic cones and outflows, and the era and extent of the great erosion.

These several topics are further discussed in separate chapters, treating of the physical history and evolution of the

* This notice has been prepared from advance sheets placed in the hands of the writer by the author.

VISHNU'S TEMPLE, COLORADO CAÑON.

various cañons of the district, their excavation, and the origin of special details.

In a previous Report on the High Plateau District of Utah, published in 1880, Captain Dutton had described with like fulness the country immediately north,* and the two Reports relate really to different parts of the same great plateau area. Between the parallels of 40° and 40° 30′, the Great Salt Lake comes to its southern end. Just east, in the same latitude, the Wasatch Mountains, rising in points to 13,000 feet, begin their north-and-south course. Farther eastward are spread out the tamer Uinta Mountains, a great plateau, 150 miles from east to west, and averaging 10,000 to 11,000 feet in height, yet reaching at one place a height of 13,694 feet. To the south, below the parallel of 40°, the Wasatch range falls off into the "High Plateaus" of Southern Utah, the Wasatch, Pavant, Awapa, Aquarius, Paunságunt and Markágunt plateaus, ranging from 10,000 to 11,600 feet in height, which are the subject of the former Report. These plateaus have together a length from north to south of 175 miles, with a breadth of 25 to 80 miles. Eocene beds, of lacustrine origin, are widely distributed over the summits with also extensive streams of trachytic and basaltic lavas of still later origin; and Cretaceous, Jurassic, Triassic, Permian and Carboniferous rocks come to view successively on descending into the intersecting gorges or valleys.

Passing south and southeast of the southernmost of these High Plateaus, the Markágunt and the Paunságunt, a series of great steps commences leading down, over successive cliffs and broad plateaus, from the Eocene table-land of the summit to, finally, the depths of the Grand Cañon—and with the account of this majestic "stairway" the new Report begins.

These successive steps to the cañon, along with the plateaus below them, cover a region on the north side of the river averaging 75 miles in breadth. The plateaus which this border area of the Grand Cañon include, beginning to the eastward, are the Kaiparowits, Paria, Kaibab, Kanab, Uinkaret, and Sheavwits plateaus.

The first step down from the Eocene of the summit is over Cretaceous rocks, for 4000 to 5000 feet—a vast series of sandstones and shales, of pale yellow and light brown to gray shades, rather brilliant in effect. The Kaiparowits plateau, the easternmost, is wholly Cretaceous at top, almost to the margin of the Colorado; and similar Cretaceous mesas cover nearly all northeastern Arizona and reach indefinitely eastward. The second step downward is over the Jura-Trias—first a descent of 300–500 feet over red shales with fossiliferous calcareous layers containing Jurassic fossils; then a great stratum of

* See for a notice, vol. xx, p. 63, 1880.

white sandstone, conspicuous for its cliffs and its massive architectural projections with few horizontal lines, a marked feature in the landscape along the Virgen. Below the "White Cliffs" come the "Vermilion Cliffs" of the Trias, mostly 1200 to over 2000 feet in height, generally thin-bedded, often presenting southward "a majestic front richly sculptured and blazing with gorgeous colors." The Paria plateau consists of the Trias at top. Below the Trias, and in some parts making the lower step of the great stairway, lies the Permian, consisting of evenly bedded sandy shales with thin layers of limestone, of deep and rich coloring—often chocolate, purple, red-brown between horizontal patches of violet, lavender and white.

Below all these, lies the Carboniferous. It is the floor of all the plateaus above enumerated except the two eastern, the Paria and Kaiparowits; and covers also a wide region south of the Colorado at the same level.

The thickness of rock passed over in descending from the top of the Eocene of the High Plateaus to the Carboniferous is 5000 to 6000 feet. But the Eocene thickens toward the Uinta Mountains to 4000 and 5000 feet, making the whole thickness of the formations overlying the Carboniferous about 10,000 feet. The Carboniferous has a small dip to the northward. The overlying beds partake of this dip in some degree, especially at their free southern borders. The beds thin somewhat to the eastward and most markedly so the Triassic in contrast with the Tertiary which thicken in that direction.

The Grand Cañon, 5000 to 6000 feet in depth, is cut out of the Carboniferous and lower formations, the Carboniferous making 4000 to 4500 feet of the whole, and the Archæan, with in some parts Silurian and Devonian strata, constituting the rest. The higher beds of the Carboniferous for 700 to 750 feet are of limestone; next below are red and gray sandstones and shales for 1000 to 1500 feet; and again below, for 1800 feet, mostly limestones, with other sandstones underneath.

The channel of the cañon has (1) an upper portion or story which is 4 to 5 miles wide, and about 2000 feet deep, and (2) an inner chasm cut through the floor of the upper to a depth of 3000 feet or more. The lofty walls of the upper portion, set back 2 to 3 miles from the profounder chasm and stretching along interminably (more than 100 miles), up and down the stream from east to west, are wonderful in architectural features, and add immensely to the grandeur of the landscapes. The so-called inner chasm is not a narrow cleft with perpendicular walls, and a slender strip of water half hid away in the dark depths. It has nowhere a width less than its depth and

is generally of much greater width; and over part of the broad area are crowds of mountain towers and temples, 3000 to 5500 feet in height, with combinations of amphitheatres, alcoves, buttresses and towers along the sides; and all is open to the sunlight.

The scenic effects of the out-cropping formations depend greatly on the horizontal bedding; but also largely on the unequal spacing of the beds, the variations in hardness tending to make cliffs to alternate with taluses, at longer or shorter intervals; and on the diversities in shade and brilliancy of coloring. The architectural forms, though on a mountain scale, are literally architectural, and different in type for the different formations. We quote a few paragraphs from the descriptive part of the Report.

"Each of the greater sedimentary groups of the terraces, from the Eocene to the Permian inclusive, has its own style of sculpture and architecture; and it is at first surprising and always pleasing to observe how strongly the several styles contrast with each other. The elephantine structure of the Nile, the Grecian temples, the pagodas of China, the cathedrals of western Europe, do not offer stronger contrasts than those we successively encounter as we descend the great stairway which leads down from the High Plateaus. As we pass from one terrace to another the scene is wholly changed: not only in the bolder and grander masses which dominate the landscape, but in every detail and accessory; in the tone of the color-masses, in the vegetation, and in the spirit and subjective influence of the scenery. Of these and many strong antitheses, there is none stronger than that between the repose of the Jura and the animation of the Trias.

"The profile of the Vermilion Cliffs is very complex, though conforming to a definite type and made up of simple elements. It consists of a series of vertical ledges rising tier above tier, story above story, with intervening slopes covered with talus, through which the beds project their fretted edges. The stratification is always revealed with perfect distinctness and is even emphasized by the peculiar weathering. Northwestward of the southern promontory at Pipe Spring, the cliffs steadily increase in grandeur and animation, and also assume new features. Near the summit of the series is a very heavy stratum of sandstone, which is every where distinguishable from the others. This member is seen at Kanab with a thickness of about 200 feet. It increases westward, becoming 400 feet at Pipe Spring. Beyond that it still increases, reaching a thickness of more than 1200 feet in the valley of the Virgen. It has many strong features, and yet they elude description. One point, however, may be seized upon, and that is a series of joints nearly vertical with which the mass is every where riven. The fissures thus produced have been slowly enlarged by weathering, and down the face of every escarpment run the dark

shadows of these rifts. They reach often from top to bottom of the mass and penetrate deeply its recesses. Wherever this great member forms the entablature—and west of Pepis Spring it usually does so—its crest is uneven and presents towers and buttresses produced by the widening of these cracks. Near Short Creek it breaks into lofty truncated towers of great beauty and grandeur, with strongly emphasized vertical lines and decorations, suggestive of cathedral architecture on a colossal scale. Still loftier and more ornate become the structures as we approach the Virgen. At length they reach the sublime. The altitudes increase until they approach 2000 feet above the plain. The wall is recessed with large amphitheatres, buttressed with huge spurs and decorated with towers and pinnacles. Here, too, for the first time, along their westward trend, the Vermilion Cliffs send off buttes. And giant buttes they verily are, rearing their unassailable summits into the domain of the clouds, rich with the aspiring forms of Gothic type, and flinging back in red and purple the intense sunlight poured over them.

"As we moved northward from Short Creek, we had frequent opportunities to admire these cliffs and buttes." * * *

"In an hour's time we reached the crest of the isthmus, and in an instant there flashed before us a scene never to be forgotten—that of the temples and towers of the Virgen. At our feet the surface drops down by cliff and talus 1200 feet upon a broad and rugged plain cut by narrow cañons. The slopes, the winding ledges, the bosses of projecting rock, the naked, scanty soil display colors which are truly amazing. Chocolate, maroon, purple, lavender, magenta, with broad bands of toned white, are laid in horizontal belts, strongly contrasting with each other, and the ever-varying slope of the surface cuts across them capriciously, so that the sharply defined belts wind about like the contours of a map. From right to left across the farther foreground of the picture stretches the inner cañon of the Virgen, 900 feet in depth and here of considerable width. Its bottom is for the most part unseen, but in one place it is disclosed by a turn in its course, showing the vivid green of vegetation. Across the cañon, and rather more than a mile and a half beyond it, stands the central and commanding object of the picture, the western temple, rising 4000 feet above the river. Its glorious summit was the object we had seen an hour before, and now the matchless beauty and majesty of its vast mass is all before us; yet it is only the central object of a mighty throng of structures wrought up to the same exalted style, and filling up the entire panorama. Directly in front of us a complex mass of white towers, springing from a central pile, mounts upward to the clouds. Out of their midst, and high over all, rises the dome-like mass of the temple which dominates the entire landscape. It is almost pure white, with brilliant streaks of carmine descending its vertical walls. The towers which surround it are of inferior mass and altitude, but each of them is a study of fine form and architectural effect. They are white above, and

change to a strong, rich red below. Dome and towers are planted upon a substructure no less admirable. Its plan is indefinite, but its profiles are perfectly systematic. A curtain wall, 1400 feet high, descends vertically from the eaves of the temples, and is succeeded by a steep slope of ever-widening base-courses leading down to the esplanade below. The curtain wall is decorated with a lavish display of vertical mouldings, and the ridges, eaves, and mitred angles are fretted with serrated cusps. This ornamentation is suggestive rather than precise, but it is none the less effective. It is neither repetitive, nor symmetrical. But though exact symmetry is wanting, nature has here brought home to us the truth that symmetry is only one of an infinite range of devices by which beauty can be materialized."

The illustrations of the report, both in the text and atlas, are admirable, and more than sustain the author's descriptions. Those from the drawings of Mr. Holmes are like photographs in accuracy of detail and aerial effect. Plate IV is a reduced copy of a plate in the text, representing "Vishnu's Temple," one of the architectural masses in the Cañon south of the Kaibab plateau, *having a height from its base of* 5500 *feet.* For others, and a continuation of the descriptions of this region of nature's architectural marvels, the reader is referred to the report itself.

The following are some points in the geological history of the region which the report brings out:

While Silurian and Devonian rocks (the latter paleontologically identified in the Kanab Cañon by Mr. Walcott) occasionally occur, the Carboniferous, for the most part, rests directly on the Archæan. Between the Carboniferous and the underlying Paleozoic beds there is always unconformability, both through the greater or less dip of the latter and the erosion of the surface: and the same is true through the Sierra country of central and western Arizona, and of Nevada and western Utah. But from the bottom of the Carboniferous to the top of the Cretaceous the beds are one continuous conformable series. The beds of this long range appear to have been deposited horizontally; the thick lower limestone of the Carboniferous was probably made in moderately deep water, and the rest of the formations—mostly sandstones and shales—near mean-tide level, as proved by ripple-marks and other evidences. From this fact is drawn the important conclusion that a gradual subsidence was taking place as the depositions went forward; and since the Cretaceous contains lignitic or coal-bearing beds throughout, and at short intervals alternating in the upper part with brackish-water instead of marine beds, it is inferred that there were alternations of emergence and submergence going on during its progress also, ending finally in

widely-extended estuary and terrestrial conditions. The Cretaceous seas probably stretched across from the Gulf of Mexico to the Pacific, with interruptions only of narrow emerged areas—that of the Great Basin being one of these, and another in Arizona. Between these two areas, or along the parallels of 34° and 37°, "we can now travel from the Mississippi to the Pacific without being at any time more than fifty miles distant from some known mass of Cretaceous beds;" and "every indication we now have raises a presumption in favor of a complete connection." The distance is nearly 2,000 miles.

"At the close of the Cretaceous, important vertical movements were inaugurated, and around the borders of the Plateau province some important flexures were generated." This epoch, the close of the "Lignitic" or "Laramie" formation, was that, it is supposed, of the making of the Wasatch Mountains. A great area, from the Aquarius plateau to the Colorado, must have been denuded at this time of the Cretaceous, so that the Tertiary over it was laid down on the Jurassic beds; and similar facts occur at other points indicating that "the Cretaceous closed amid important disturbances. Still the deposition of strata was not ended." The depositions of Eocene beds went forward in *fresh* waters until 1200 to 5,000 feet of strata were added to the series, the facts showing that a single lake extended from the Uinta Mountains southward over the Plateau region; that the elevation was sufficient, and sufficiently persistent to make great lake-basins; and evincing, also, since the beds are successively of shallow water origin, that, as before, a subsidence was going on over the Eocene lake region to as great a depth as the thickness of the beds. The earlier subsidence had been greatest in the line of the Wasatch Mountains; the later was most so farther to the northeastward, in the region of the Uinta Mountains, where Eocene beds have a thickness of 5,000 feet and the great Eocene lake made its final disappearance, only the lower Eocene occurring over the Plateau District.

The Miocene depositions followed, but it is difficult to find "in this district any epoch separating the later Eocene from the Miocene; and of the Pliocene nothing is known from any beds or fossils. Both eras were no doubt eras of gradual emergence; at the close of the Miocene an uplift took place of probably 2,000 to 3,000 feet, and at or near the close of the Pliocene a greater upheaval of 3,000 to 4,000 feet."

The making of the great north-and-south faults of the plateau region—first described by Powell—some of which extend northward over the region of the "High Plateaus" far toward the Wasatch Mountains, began, according to the author, at the close of the Miocene, and went on for some of them during the

Pliocene. Among them the largest is the Hurricane fault, following the west side of the Uinkaret plateau, along which the displacement in the south wall of the Grand Cañon is 1500 feet; and farther north, near the Virgen River, 6,600, it making the Hurricane bluff 1200 to 1600 feet—ten miles farther north probably over 12,000 feet, the Eocene being on one side and the Carboniferous on the other—that is, if, as is probable, the Permian and Mesozoic beds, which there are now absent, were once in the series as they are elsewhere; and, farther north, it extends into Utah, along the west side of the Markágunt plateau and the southwest flank of the Tushar range, where it finally ends its course of 200 miles beneath great lava floods. Another is the Kaibab fault which outlines the Kaibab plateau on the west, and extends to the northward about as far as the Hurricane fault. The Kaibab fault is referred to the Pliocene and the Hurricane to later Pliocene.

The numerous volcanic cones and wide spread streams of lavas add greatly to the geological interest of the region. They cover much of the Uinkaret plateau, and descend from it for 1500 to 2000 feet into the upper portions of the Grand Cañon, and thus prove that the eruptions took place after the great faulting and after a large part of the erosion. But beyond this, some of the streams make a further plunge to the depths of the cañon; and in places over the bottom also there are cones and widely spread sheets of lavas. The Miocene, or its close, was the era of the earlier volcanic out-flows; but a large part were of Pliocene date; and some are of quite recent time, their scoriaceous lavas looking as if of recent ejection, perhaps dating but a few centuries back.

In the history of the erosion, Captain Dutton observes that the first channeling along the line of the Grand Cañon occurred in the early Tertiary or at the close of the Cretaceous. The removal of Cretaceous beds that once covered the borders of the Colorado Cañon south of the Aquarius plateau, leaving a surface of Jurassic beds, was part of the earlier erosion. The emergence was small in the Eocene, the climate moist, and, during its latter part "the degrading forces no doubt made progress in removing the Mesozoic deposits that originally covered the region;" and this work went on through the Miocene, completing nearly the denudation of the Tertiary, Cretaceous and Jura-Trias; that is, sweeping off large parts of these upper formations down to the Permian. The cutting of the Grand Cañon through the Carboniferous is stated to have gone on after the uplift closing the Miocene; at this time "the outer chasm of the Grand Cañon was cut" through the Permian and the upper part of the Carboniferous, and although it must at first have been narrow, it finally reached a width of some miles.

Then after another rise, "most probably near the close of the Pliocene," "the faults increased their displacement, the volcanic vents reopened," and the Colorado River resumed the operation of sinking its channel; and, as time went forward it made the inner chasm with all its grand features. The erosion made quite rapid progress owing to the high pitch given the surface by the new elevation; it has now almost ceased, the river having nearly reached a level of slight change by erosion or what is here called its base-level,—a base-level nearly for the stream at the present state of elevation of the land and ordinary annual floods. Captain Dutton observed that the Glacier period intervened between the Pliocene and the present time—a rainy rather than an icy era for that district—increasing the depth of the Cañon and making some new channels; but, he adds—limiting the effects more than some others might do, in view of the fact that the head waters of the Colorado drain the wide mountain region of the Rocky mountain summit for a breadth from north to south of more than 300 miles,—"The Glacial period appears to have been of too brief duration to have achieved any very great results in this district."

J. D. D.

ART. X.—*The relative Temperatures of the two Hemispheres of the Earth;* by WILLIAM FERREL.

IT was once thought, and the idea still prevails almost universally, that the southern hemisphere of our globe is colder than the northern. This first arose from the comparison of Dove's thermal charts for the two hemispheres. The observations upon which that of the southern hemisphere was based extended only as far south as the parallel of 40° S. The comparison, therefore, between the two hemispheres could be made for the portions only between the equator and the parallels of 40°, and so far as this comparison extends, the mean temperature of the southern is really greater than that of the northern. It was, however, first suggested by Dove, and rendered still more probable by the researches and theoretical considerations of Hopkins, Sartorius von Waltershausen, Forbes and others, that the conditions may be reversed in the higher latitudes, and the mean temperature there be greater in the southern hemisphere than in the northern.

With a view of settling this question, and that of the equality or inequality of the mean temperatures of the two hemispheres, Dr. Hann of Vienna* has recently undertaken a

* Ueber die Temperature der südlichen Hemisphäre. Sitzb. der k. Akad. der Wissensch., B. lxxxv, 1882.

new discussion of the subject, availing himself of all the more recent observations made in the lower latitudes of the southern hemisphere, especially of those made by the Venus expeditions to the islands of Kerguelen, Auckland and St. Paul. With the aid of these additional observations he obtained as an expression of the mean temperature of the southern hemisphere, in centigrade degrees,

$$T = 26°{\cdot}0 + 6°{\cdot}94 \sin \varphi - 42{\cdot}28 \sin^2 \varphi$$

in which φ is the latitude.

By the integration of the temperature, given by this expression, over the whole surface of the hemisphere, the average is found to be 15°·4. From a comparison of this with the result obtained by the writer* for the northern hemisphere, 15°·3, Dr. Hann concludes that the mean temperatures of the two hemispheres are probably exactly the same.

Using exclusively only ocean stations of observation, he obtained, instead of the expression above, the following:

$$T = 26°{\cdot}0 + 10°{\cdot}03 \sin \varphi - 47{\cdot}13 \sin^2 \varphi$$

From this expression the mean temperature of the southern hemisphere was found to be 15°·2, very nearly the same as found from the other

The comparison of the temperatures, given by the first of the preceding formulæ for the several latitudes, with those obtained by Forbes for the northern hemisphere, is as follows:

Latitude,	40°	45°	50°	55°	60°
S. hemisphere,	13·0	9·8	6·5	3·3	0·3
N. hemisphere,	13·6	9·7	5·8	2·3	−1·2
Difference,	−0·6	+0·1	+0·7	+1·0	+1·5

It is seen from these differences that, at about the parallel of 45°, the southern hemisphere begins to be warmer than the northern, and as Dr. Hann now considers the temperatures at the latitudes 40° to 50° to be pretty well determined, he thinks it leaves the result with scarcely a doubt. It seems, therefore, to be now well established that, although the temperatures of the lower latitudes of the southern hemisphere are less than those of the same latitudes of the northern hemisphere, yet it is the reverse in the higher latitudes, and that there is no sensible difference between the mean temperatures of the two hemispheres.

The reason of this becomes clear when we consider that if the surface of the earth were all land, the difference between the temperatures of the equatorial and polar regions would be much greater than if the whole surface were water. In the former case there would be no transfer of heat by means of

† Meteorological Researches, Part I; U. S. Coast Survey Report, 1875.

ocean currents from the equator toward the poles, while in the latter the well-known gradual interchange of water between the equatorial and polar regions, arising from a difference of temperature, conveys a great amount of heat from the former to the latter, diminishing the temperature in the lower latitudes and increasing it in the higher ones. It has been estimated that the heat transferred from the torrid zone to higher latitudes by the Gulf Stream alone amounts to about one-twelfth of all the heat received by the earth from the sun between the equator and the tropic of Cancer, and that transferred by the Kuro-siwo to more than twice as much more. If all the heat received by the torrid zone had to be disposed of by radiation back into space, of course the temperature there would be higher, since by the law of radiation, where greater radiation is required, there must be a higher temperature of the radiating surface. On the other hand, if no heat were received in the polar zone, or higher latitudes, except that received directly from the sun, the temperature at which the radiating surface would have to stand to dispose of the heat received would be less. We see the effect upon temperature of this transfer of heat from equatorial to polar regions in the relative mean temperatures of land and water in the northern hemisphere. In the lower latitudes mean ocean temperatures are lower, and in the higher latitudes greater, than on land. And the differences would be still greater, if much of the heat conveyed by ocean currents to the higher latitudes were not transferred to the land by the general eastward motion of the air in those latitudes.

Since the amount of transfer of heat from equatorial to polar regions must be somewhat in proportion to the amount of ocean surface, the amount of this transferrence must be greater in the southern than in the northern hemisphere. Hence the difference between the mean temperature of the equatorial and polar regions must be less in the southern than in the northern hemisphere. If the mean temperatures of the two hemispheres are equal, as has been shown, it is readily seen that the effect of this must be to make the temperature of the lower latitudes colder, and that of the higher latitudes warmer, in the southern hemisphere than in the northern.

It is well known that the annual amount of heat received from the sun by each hemisphere is precisely the same. A number of hypotheses have therefore been devised to account for the supposed difference of temperature between the two hemispheres. Poisson supposed it was due to the greater heat-absorbing power of land than of water, which, on account of the greater proportion of land in the northern than in the southern hemisphere, would cause the temperature of the

former to be a little the greater. But if the principle of the equality of the absorbing and radiating powers of all bodies is true, which is generally conceded, thus a greater absorbing power, if it is accompanied by a proportionately greater radiating power, cannot give rise to a higher temperature, and to account for any difference in the temperatures of the two hemispheres, it would be necessary to suppose that the absorbing and radiating powers are different, either in a land-, or in a water-surface, or in both.

The establishment, therefore, of the equality of the mean temperatures of the two hemispheres seems to confirm the principle of the equality of the absorbing and radiating powers of bodies, since this seems to be the case with regard to water, and land with all its variety of surface. If this were not true for any considerable part of the earth's surface, it would affect the equality of the mean temperatures of the two hemispheres.

ART. XI.—*An Air-thermometer whose indications are independent of the Barometric Pressure;* by ALBERT A. MICHELSON.

THE appearance of an abstract of a paper by Pettersson* on a new air-thermometer, has led me to publish, sooner than I had contemplated, a notice of an instrument far simpler and more manageable than that which is there described, and which likewise retains the important advantage of giving indications which are independent of the external pressure.

The instrument consists of a glass bulb and stem, the former about 40^{mm} and the latter about 2^{mm} in interior diameter. The bulb contains dry air at a pressure of about 100^{mm} of mercury, and this air is separated from the upper portion of the tube by a column of mercury about 100^{mm} in length. The mercury remains above the air, notwithstanding the large diameter of the bore, owing to the resistance to deformation of the meniscus. The space above the mercury is a vacuum.

Thus the pressure of the air in the bulb is constant and is equal to that of the column of mercury above it. If the bore of the stem is not of uniform section the length of the column will change—but this length is easily read off and gives at once the true pressure.

The pressure need not be limited to 100^{mm}, but if it be much greater the instrument becomes inconveniently long.

The only precaution to be observed, beyond what is used in an ordinary mercury thermometer, is that the stem must be kept vertical.

Case School of Applied Science,
Cleveland, O., July 5th, 1882.

* Annalen der Physik und Chemie (Beiblätter), No. 5, 1882.

ART. XII.—*The Bearing of some Recent Determinations on the Correlation of the Eastern and Western Terminal Moraines;* by Professor T. C. CHAMBERLIN.

FOR several years a group of geologists of the interior have been engaged in tracing out an extensive range of terminal moraines. This may be said to have become a definitely organized movement, and to have already accomplished, so far as the interior is concerned, its main purposes, though many details remain to be worked out. The character and brevity of this note forbid any attempt to set forth in detail the respective parts played by the different investigators who have contributed to this work. In a paper entitled, "On the Extent and Significance of the Wisconsin Kettle Moraine," presented to the Wisconsin Academy of Sciences, in February, 1875 (printed in 1878), the writer attempted to collate and duly accredit everything published at that time having any specific bearing on the subject, whether definitely recognized as such by the authors or not. To a considerable extent the correlation and interpretation of the investigations of others were based upon personal visitation of characteristic and significant portions of the formation in the seven interior States traversed by the moraine. Of the much more considerable work which has been done since, the writer hopes to find an early opportunity to speak fully.

The result of these investigations has been the determination of a morainic belt of extraordinary extent and character. It traverses portions of Ohio, Indiana, Michigan, Illinois, Wisconsin, Minnesota, Iowa and Dakota, and extends a distance, as yet undetermined, into the British Possessions. Presumably it crosses the continent. It will be convenient to speak of this as a single moraine, though in reality it is a complex range, formed of two principal and one or more subordinate morainic ranges, which sometimes coalesce and sometimes separate, so as to embrace a belt twenty or thirty miles in width.

It is important to our present purpose to observe three salient facts that characterize it.

1. Instead of pursuing a somewhat direct course across the country *it is disposed in large loops which have for their axes the great valleys of the interior.* These loops represent the margins of great glacial lobes that fringed the greater ice-sheet of the north. Of the ten great lobes now determined, one occupied each of the following valleys, viz: of the Dakota River, of the Minnesota River, of the western projection of Lake Superior, of the Chippewa River, of Green Bay, of Lake Michigan, of Saginaw Bay, of the Maumee, of the Scioto, and of the Grand

River, the special interest of this discussion attaching to the last.

2. *The moraine does not lie upon the margin of the glaciated area*, but is distant from it at considerable, though varying distances.

3. *The moraine marks a second glacial advance separated from the former by a considerable interval of time.* From the consideration of several distinct lines of evidence, the impression has been gained that the interval which separated the earlier from the later glaciation, was equal to, or greater than, that which has elapsed since the latter. But the correctness or otherwise of this is unimportant to our present purpose.

Somewhat later a similar attempt was made by eastern geologists to trace out an analogous moraine in the coast region. The result of their investigations was the determination of a massive morainic belt traversing northern New Jersey, the entire extent of Long Island, and the smaller islands lying to the eastward, striking the Atlantic on the peninsula of Cape Cod. Concerning this it is likewise essential to note two characteristics and a negation.

1. Unlike the western moraine, *it is not disposed in conspicuous loops*, as now delineated. This is the more to be remarked, since the surface inequalities of the region lying north of it are greater than those of the interior.

2. *This moraine marks the southern limit of the drift-sheet.*

3. It has not yet been determined whether this moraine represents the limit of the earlier or later glacial advance.

Notwithstanding their differences, the characteristics of the eastern and western moraines are so strikingly similar, that their tentative correlation as portions of a single moraine presented itself on the first appearance of Professor Cook's results, and was given expression to in the paper above cited. The correct correlation of these moraines and the decisive determination as to whether the coast member belongs to the earlier or later period of glaciation, are manifestly questions of great interest, and the purpose of this paper is to make a preliminary contribution toward their solution. The hypothesis of their unity is somewhat strengthened by the fact that thus far no similar moraine has been traced along the margin of the drift in the interior. It may be, however, unsafe to assume the absence of such moraine until the region shall have been critically examined with especial reference to this question.

During the past year Professors Lewis and Wright, of the Pennsylvania Survey, have been engaged in tracing a marginal moraine from the western terminus of the New Jersey range, across Pennsylvania. Their results are not yet published, and will be awaited with interest.

Meanwhile the investigations of the writer, which have been pursued, though with considerable interruption arising from the pressure of other duties, since 1873, have developed some evidence having an important bearing on the correct correlation of the eastern and western moraines. In the paper above referred to attention was called to the fact that, while in the immediate Mississippi valley the kettle moraine lies some hundreds of miles back from the drift margin, in Ohio it approaches it much more closely, and this fact was adduced in support of the presumption that the kettle moraine might become marginal to the drift area somewhere farther east.

In the reports of the geological survey of Ohio a series of peculiar drift accumulations, consisting largely of gravelly hills, are described as occupying the water-shed between the Ohio and Lake Erie. The identification of these with the kettle moraine made* on the basis of the descriptions of the several geologists who have taken account of them, interpreted in the light of some personal observation, is in the main confirmed by further investigation. But, whatever may be true of the kame-like gravelly hills, the course of the moraine diverges quite widely from the summit line of the water-shed. In conformity to its habit, it is disposed in loops, one of which has for its axis the Maumee valley, another the Scioto, and a third, in eastern Ohio, with which we are more especially concerned, the Grand River valley. It is worthy of note that these axes correspond to the three hydrographic sections of Lake Erie, as marked by its surface outline, but more significantly by the soundings of the Lake Survey.

The interest which attaches to the eastern loop in its bearing upon the correlation of eastern and western moraines lies in the fact that in its southern portion it constitutes the margin of the drift-bearing area. Its geography may be briefly sketched as follows: Beginning with the eastern marginal moraine of the Scioto lobe, in the southwestern corner of Stark Co., it pursues a north-northeasterly course to the northern part of Portage Co. In this portion, the moraine attains a more pronounced development than in adjacent regions, and has been made the basis of the descriptions and illustrations of Dr. Newberry† and Colonel Whittlesey.‡ In this it conforms to a law observed to be widely prevalent in its westerly extension, viz: that the reëntrant portions lying between two glacial lobes have a stronger development, and are more especially characterized by the knob-and-basin features, and gravelly constitution, than those portions which were simply marginal

* Paper cited, pp. 21–25. † Geol. Survey of Ohio, vol. ii, pp. 41–46.

‡ On the Fresh Water Glacial Drift of the Northwestern States, Smithsonian Contributions, p. 6.

to the glacier. The northerly part of this belt is such an intermediate moraine, formed between the Scioto and Grand River glaciers. The moraine proper to the Grand River glacier diverges from this common intermediate one in the north central part of Stark Co., from which point it curves rapidly to the eastward, and passes into Columbiana Co. in West and southern Knox townships. Thence it passes eastward in an undulating course through the northern portion of Columbiana Co., from which it enters Pennsylvania. The latter state being under investigation, the course of the moraine was not pursued by me further than to determine its general northeasterly direction and persistence. This latter portion marks the left hand margin of the glacial lobe.

These determinations are thoroughly in harmony with, and bring into rational unity the interesting observations of Mr. M. C. Read,* on the direction of glacial striation in northeastern Ohio. Indeed my working hypothesis was based upon them. These striations show a remarkable divergence from the axis of the Grand River valley toward the high lands that form its rim, and, as it proves, toward the margin of the glacial lobe that gave rise to them. This conforms to the law of divergent internal movement, first demonstrated by myself in respect to the Green Bay glacier, and since shown to be a general character of the glacial lobes of the interior, and will doubtless prove to be a uniform feature of glaciers deploying in an open country. That the southern extremity of this lobe reached the margin of the drift area is affirmed by the observations of Dr. Newberry, on Columbiana Co.,† who asserts that the drift is confined to the northwestern portion of the county, and of Professor J. J. Stevenson on Carroll Co.,‡ who says that after diligent examination, no drift was found except a few doubtful specimens in the northeast, and that the boundary line of drift influence lies to the north and northeast of the county.

The outer margin of the moraine, according to my determination, approaches to within less than three miles of the northern line of Carroll Co., and I observed no apparent evidence of glaciation south of it. A little farther east, however, drift was observed from three to four miles south of the apparent margin of the moraine. Whether this was formed contemporaneously with the moraine or during the earlier glacial epoch, seems to me uncertain, but the general fact that the moraine here reaches the essential limit of the drift-bearing area, is satisfactorily determined. This removes one of the

* Geol. Survey of Ohio, vol. i, p. 531.
† Geol. Survey Ohio, vol. iii, p. 90. ‡ Ibid, p. 179.

apparent distinctions between the moraines of the coast and interior.

While, however, this distinction has been removed, the remaining one has been intensified, for it is shown that the lobate character of the glacial margin prevailed thus much farther east, and as that character is manifestly due to topographical features, it becomes all the more remarkable that in the more diversified region of the east, the glacial outline should become more nearly uniform and rectilinear, as determined by the geologists of that region.

In New York, morainic accumulations identical in character with the class under consideration have an extensive development, but their relations and connections are not yet fully determined. The more pronounced character of the topography manifestly made itself felt upon the margin of the ice, and gave rise to local modifications, and seemingly to independent local moraines that increase the difficulties of a safe interpretation. But it is certain that, in part, the main morainic accumulations are considerably removed from the margin of the drift, and indicate that alternate divergence and approach may be found to characterize the region lying east of the point of first contact. In view of the fact that the wide separation in the Mississippi Valley is entirely closed in eastern Ohio, the suggestion that the later advance actually passed beyond the earlier in the coast region, may be worthy of entertaining as a working hypothesis. It may be further remarked that if there be alternating contact and divergence of the margins of the two glacial sheets, that a considerable belt lying back from the drift limit requires investigation.

In view of the further fact that the great moraine of the interior is found throughout the wide extent through which it has been traced, to be persistently disposed in loops, the presumption of a similar outline, attended by reëntrant angles and intermediate moraines, ought only to be dismissed when found unsupported by evidence after diligent search.

It is hoped that the foregoing suggestions at this stage of the investigation, while yet final conclusions are unformed and opinions still plastic, may not be without service to the increasingly large number of workers in this somewhat new field.

ART. XIII.—*On the Flood of the Connecticut River Valley from the melting of the Quaternary Glacier;* by JAMES D. DANA.

[Concluded from p. 373, volume xxiii.]

6. *The question as to the Elevation of the Land.*

THE remaining question in connection with the discussion respecting the Connecticut Valley during the era of the great flood is that relating to its apparent depression at the time:—Whether the change in pitch, which was proved to have been a fact, was due to a change in land-level, or only to a change in sea-level.

To understand the events of the Glacial era and that following and reason correctly on the facts, we should know which of these views is right, and, in order to know, take evidence from the region.

Toward this end, we may first compare the requirements of an hypothesis which refers the change to a change in sea-level with the facts observed. If the facts do not accord with the demands of such an hypothesis, we are then free to adopt the other view—that the depression of the land was actual and not merely apparent.

It should be understood, in advance, as a fact in terrestrial physics, that any rise in the ocean's level increasing northward produced by a change in the position of the earth's center of gravity would be,* effectively, a change (1) in the height of the land; and (2) a change in the pitch of its surface; and, New England, like the rest of the globe, being within the area so affected, it would be a change in pitch for the Connecticut Valley and the region around, as well as for the coast region. For, the ocean's surface is the reference-plane of horizontality as well as elevation, and the attraction determining its level would affect not only the land adjoining but also all instruments used over it for obtaining horizontal or perpendicular lines.

Only one source of a change in the position of the earth's center of gravity has been suggested in this connection—that from the forming of a polar ice cap of great thickness, during a glacial era in one hemisphere or the other, as in the hypothesis of Adhemar, adopted by Croll and others, this cause leading to an increased pole-ward accumulation of the oceanic waters in the ice-covered hemisphere.

The following are among the facts bearing on this subject.

(1.) There is no correspondence between the amounts of change deduced from observations and those required by the

* Assuming that the earth is so far rigid that it would not suffer deformation—or a depression of the crust—from the weight of the ice resting on its surface.

hypothesis. Since the amount of the apparent depression, that is, of the rise in the earth's curving water-surface which would follow from such a change in the position of the center of gravity, would increase northward very nearly in the ratio of the sine of the latitude, it follows that if the amount at Montreal were 520 feet, as the facts reported show, they should have been about 507 feet at Lewiston, Maine, 491 feet at Point Shirley, Mass. (near Boston), and 480 feet along the north shore of Long Island Sound. But, instead of the amounts 507, 491 and 480 feet, the actual levels observed are 200, 75 or 80, and 25 to 15 feet.*

Further, the hypothesis,—calculating again from the Montreal level, 520 feet,—would give hardly 730 feet for the region about the North Pole, and 720 for latitude 81° to 82°, or that of Grinnell Land and Northern Greenland; while in the latter region, Feilden and De Rance found sea-shells (*Pecten Grœnlandicus, Astarte boreale, Mya truncata, Saxicava rugosa*, etc.) in beach-made deposits at different levels up to 1000 feet.† But this is not all the divergence of the facts from Greenland terraces. For, in the part of Danish Greenland called Southern Greenland (between the parallels of 60° and 67° 40′), the part best known, no heights of terraces or elevated beaches have been reported above 350 feet. Dr. Rink, one of the Greenland explorers as well as Government Inspector for many years of Southern Greenland, mentions, in his latest work on Danish Greenland (1877), the occurrence, in this part of the semicontinent, of terraces at a height of 100 feet, rising in some places to 200 feet, and nothing of higher level. Mr. A. Kornerup, Geologist of Lieutenant Jensen's Expedition of 1878,‡ observed terraces at several points, and describes a series, near the parallel of 63° 10′ N., to the north of Fiskernaes, the highest of which was 101 meters, and another in 63° 5′ N., of 106 meters (348 feet) as the maximum height. Nordenskiöld, in connection with his exploration in the vicinity of Jakobshavn, in 1870,§ observed shell beaches up to a height of 100 feet near 69° 10′ N. Hayes, in his "Open Polar Sea," gives (p. 402) 110 feet as the height of terraces at Port Foulke, near 78° 10′ N., north of Cape Alexander. Kane describes, in volume ii of his Arctic Explorations (p. 80) a series of terraces in 78° 40′, the highest of which was 480 feet above the sea. Three degrees in latitude north of the last region occur the beaches at 1,000 feet, mentioned above. The discrepancy is

* The height of the beds at Point Shirley is made 75 or 80 feet by Professor Shaler in his recent quarto volume on Glaciers forming the first volume of his proposed "Illustrations of the Earth's Surface."

† Quart. J. Geol. Soc., xxxiv, 563, 1878.

‡ Meddelelser om Grönland, 196 pp. 8vo, Copenhagen, 1879.

§ Geol. Mag., 1872, p. 400.

thus increasingly great on going southward along the Greenland coast.

It may be said that the facts from Greenland are only partially known; and, again, that a depression is now going on in Southern Greenland which has increased, and may have occasioned, the discrepancy. But, connecting them with the facts from the Atlantic borders in more southern latitudes, the evidence against the hypothesis is decisive.

(2.) The idea of a polar ice-cap of the extent claimed is an assumption opposed to known meteorological laws and observed climatal facts. For the position of the region of maximum ice would have depended very largely on that of the area of greatest precipitation; and, as the writer, accordingly, some years since suggested,* and Mr. W. J. McGee has formally demonstrated,† the ice would have diminished toward the pole as well as to the northwestward. The eastern ice-range, located in this way between the Atlantic Ocean and points not far west of the Winnipeg line of lakes, 1000 miles in width at base, presented an immense surface for condensing the moisture of the Atlantic winds and diminishing the amount carried northward, so that the ice in Greenland would have had hardly half the height of that to the southwest, and more northern polar regions still less;—Mr. McGee's calculations making the thickness in Greenland in 60° N., 5728 feet, and in 70° N., 2800 feet, while south and southeast of Hudson's Bay on the Canada water-shed, it was probably not less than 12,000 feet.

In accordance with these conclusions, the ice, at the present time, is reported by Arctic travelers to be less thick in the northern part of North Greenland than in the part to the south, and also in the lands west of Greenland than on Greenland itself. Messrs. Feilden and De Rance (loc. cit., p. 567) speak of the paucity of glaciers in Grinnell Land, lying just west of Greenland, stating that north of 81° N. on this more western land, no glaciers descend to the sea-level although they do on the coast opposite of Greenland, the situation of Greenland against the Atlantic rendering it a region of more precipitation than that to the west; and so it would have been under the more favorable conditions for precipitation of the Glacial era. As to the height of the Greenland ice in the Glacial era we have the observations of Mr. A. Kornerup, of Jensen's Expedition,‡ that, near the parallel of 64° N. about the Ameralik and Buxe fiords, there are glacial scratches at a height of 1260 meters

* This Journal, III, v, 206, 1873, ix, 312, x, 385, 1875, xiii, 79, 1877, xv, 250, 1878.

† Proc. Amer. Assoc., xxix, 1880, on Maximum Synchronous Glaciation; a portion of which paper on the particular point above referred to is cited in vol. xxii, of this Journal, p. 264 (1881). Mr. McGee gives for the thickness at 50° N. only 8213 feet.

‡ Loc. cit., pp. 109–113.

(4134 feet), a level almost 3000 feet above any glaciers now in that vicinity, and near the parallel of 63° N., in the vicinity of Kuvnilik and Björnesund, at heights of 940 to 1100 meters (the latter 3609 feet); but none on the upper part of Nukagpiarsuak, northwest of Kuvnilik, whose height is 1520 meters (4987 feet). Mr. Kornerup also states that in the era of extreme glaciation when the glacier was 3000 feet higher than now, the movement of the ice was nearly *east-and-west*, but subsequently, as the scratches at lower levels show, it followed the direction of the fiords or valleys:—a change evidently due to the thinning of the ice, the pitch of the upper surface being great enough when the glacier was at its maximum to cause ice to move independently of the courses of valleys or depressions beneath, and not so after the thickness had been much reduced. This fact had its parallel all over glaciated North America.

(3.) In addition, the hypothesis makes the submergence of the Coast region (indicated by elevated beaches) to have taken place *during the Glacial period, and to have passed its maximum in the height of the period;* when, according to the facts, whatever the condition in the Glacial era, the submergence was a prominent feature of the era when melting was going forward and the ice finally disappeared—the Champlain period. This point needs no special remark after the descriptions already given of the Connecticut River terraces, and the explanations in the following part of this paper.

(4.) But the Glacial era was not for the higher latitudes generally one of *less* elevation in the land than now, and was probably one of somewhat greater elevation for large portions.

The arguments in favor of such elevation I here briefly review, in order to test them by a reference to recent discoveries.

(*a*) One of these arguments is based on the depth to which many river channels are excavated below the present bed of the stream. It has been strongly urged by Dr. Newberry and others. The facts supporting it have been drawn from New England and the States of New York, Pennsylvania, Ohio, Indiana, Illinois, Wisconsin, and from British America; and new cases are annually becoming known. The Pennsylvania Geological Report for 1881 by Mr. I. C. White, treating of Erie and Crawford Counties (among the western counties of the State), remarks that a boring for oil on French Creek, below Meadville, descended *for* 285 *feet through the drift;* and another, in Conneaut Creek valley, 180 feet in drift; and in connection with his account of these and other cases, he speaks of it as a general fact that "the present water courses meander along the upper surfaces of drift-deposits which fill the ancient valleys to various heights above the old rock beds." Other

similar facts from western Pennsylvania are presented in the report of Mr. J. F. Carll, for 1880. During 1881, Mr. J. W. Spencer published facts respecting a buried channel between Lake Erie and Lake Ontario, which entered the latter lake along the steeply escarped Dundas Valley.* He states that in this valley the drift has been penetrated to a depth of 227 feet below *the level of the lake* without reaching a rocky bottom, and that the depth of the drift is probably as much as 1,000 feet. He also points out that the channel of Lake Ontario, which has its greatest depth abruptly near the southern side and gradually shallows northward, is a channel of erosion and probably of cotemporaneous erosion with that of the Dundas Valley.

Reported examples of this kind are so numerous that they are regarded now as representing a general fact. Such excavations could not have been made by running water while the land was at its present level; and much less could they have been made when the land was lower than now. Their origin was hence before the Champlain period in *Glacial* or *pre-Glacial* time.

The accounts speak of these deeply-eroded valleys as filled with *drift;* and if with true *northern drift*, as is implied by the language—a point which in all cases needs special examination—they were open to their bottom to receive the drift during the Glacial era. For otherwise they would be found filled in each case with the sand and gravel of the drainage-area instead of with material from a more northern source. And if open in the Glacial era, the land was at a higher level than in the Champlain period, or that of great deposition; high enough for a flowing stream to have kept the trenches clear of deposits. Many of these valleys, like that of Dundas Valley, have a different course from that of the movement of the glacier, and hence, no aid could have been afforded by the glacier in the excavation if the level was as now; and the aid would have been ineffectual whatever the course.

The facts thus prove that if the material filling these buried valleys is true drift, the land in the Glacial era was higher than now; and *much* higher, if the drift of the Dundas Valley is 500 to 1,000 feet deep. The valleys may have been pre-Glacial in origin, but their depth would have reached its lowest limit from the latest erosion or that of the Glacial era.

The argument from the deep river-made channels intersecting sea-border regions, and now occupied by the sea—that is from long bays and fiords 100 to 3,000 feet or more in depth of water—which characterize the shores in the higher latitudes on all the continents, north and south, still stands good so far

* Proc. Amer. Phil. Soc., 1881, and Proc. Amer. Assoc. Adv. Sci., 1881.

as this at least, that if, as is probable, antedating in first origin the Glacial era, the final deepest erosion took place at that time. Being proof of water erosion, they are proof of the emerged state of lands where now are seas 1,000 to 3,000 feet deep. But they do not prove that there may not have been several successive emergences and Glacial eras concerned in their production.

(*b.*) Again, there are deserted water-courses which appear to owe their desertion to a change of level which took the flow from the waters. In many cases the desertion was due simply to a decline of the flood, and a filling of channels by the depositions. But in other cases, like that of the discharge of the St. Lawrence from the Great Lakes, an appeal to diminished southward pitch in the land is necessary to account for the depositions of the Champlain period, and also for the present condition of the lakes as to outlet.

(*d.*) An argument for the probability of a greater elevation in the Glacial than Champlain period I have based, in earlier papers, on the progressive elevation of the continent which was going on during the preceding Tertiary era; and this has lost none of its force by recent discovery. The elevation of the Rocky Mountain region from Mexico on the south to the Arctic seas, and which amounted to 10,000 and 11,000 feet in the higher portions of the United States, was not completed until the close of the Pliocene—the vast Pliocene fresh-water lakes proving this; and the close of the Pliocene was the beginning of the Glacial era. Besides this upward movement in the western two-thirds of the continent, a smaller took place in its eastern portion, as geologists have inferred from the absence of marine Tertiary either above or at the sea-level north of Cape Cod, and of Pliocene Tertiary to a large extent south of it. Thus the Tertiary changes of level were, in the main, upward to the end of this age.

It is evident, too, that these changes of level in the Tertiary were changes of land-level. For changes due to a transfer of the ocean's water meridionally would have been alike on the two sides of the continent.

It is deserving of consideration also that an elevation of large portions of the Arctic regions would be favorable to the production of Glacial conditions, for it would diminish the depth of the arctic seas, and consequently would diminish the volume of arctic currents flowing southward, and of tropical waters reaching the arctic; and it would hence increase the cold of arctic seas and lands and of the lands south, and the warmth and rate of evaporation of the North Atlantic in temperate latitudes.

The evidence reviewed thus shows that there were real

changes of land-level and not simply a change of ocean-level connected with the coming on and disappearance of the ice of the Glacial era; and that the level during the Glacial era was not below the present, like that of the era following, but at least as high as the present, and probably, in many portions of the colder latitudes, somewhat higher than now.

It thus follows that a change of level of very wide continental range introduced the era of depression or Champlain period—a change so great and so marked in its effects that it is reasonably recognized as a time-boundary in Quaternary history; and as the Champlain period was one of ameliorated climate, it may have begun with the beginning of the melting. As I have shown in the earlier part of this memoir, the depression was certainly a fact during the great flood, and began before the melting had far advanced. The fact of a warmer climate in the Champlain period is manifested in the distribution of the quadrupeds and forest trees of America, Europe and Siberia, as now generally admitted. It is most strikingly demonstrated among the lands of the Arctic seas, where, on Banks's Land (74° 48′), Prince Patrick's Island (76° 12′ N.), and elsewhere, unaltered trunks of modern fir trees, single and in forest-like accumulation, exist.

But the facts as to amount of change of level are not so well known that we can mark off the limits of the areas of elevation and depression over the higher latitudes. They do not enable us to decide whether there were not, extending northward, a series of upward and downward flexures, with only a greater general emergence than now in the Glacial era and a greater general submergence in the era following. The heights of terraces on the coast of Greenland seem to be an indication as to one in this series of flexures. In any case the facts do not sustain the ordinary assumption that the amount of depression in the Arctic regions was approximately alike in all parts, and they leave it to be proved that all portions participated in the subsidence.

The cause of the depression of the land, or of the previous elevation, this is not the place to consider.

ART. XIV.—*On the Retardation of the Maxima and Minima of Air-pressure at High Stations;* by H. A. HAZEN, A.M.

[Communicated by permission of the Chief Signal Officer of the United States Army.]

IN his tenth paper, published in the number of this Journal for January, 1879, Professor Loomis advanced certain evidence to show that, apparently, the progress of a storm center was much more rapid at the surface of the earth than at elevations above it. Many arguments have been advanced by others for and against this theory. It is the purpose of this article to put forth certain facts which have come to light, and which it is hoped will tend to elucidate the subject.

Many years ago it was noticed from hourly observations at Zurich and at the summit of the Rigi, that while the morning maximum, in the diurnal range of air-pressure, occurred at the lower station at ten, it did not occur at the summit of the mountain until two P. M. Professor Loomis writes (this Journal, January, 1879, pp. 11 and 12): "Over the United States both the maxima and minima" [of accidental fluctuations] "of atmospheric pressure generally occur first near the surface of the earth, and they occur later as we rise above the surface, the retardation amounting to one hour for an elevation of from nine hundred to thirteen hundred feet." He says again (pp. 13 and 14): "The diurnal movements of the barometer exhibit a peculiarity similar to that found for the accidental fluctuations. The principal maximum occurs at the base at half past eight, but on the summit it does not occur until noon, being a retardation of three and a half hours, which is almost identically the same as we have found by a comparison of the accidental fluctuations." He says further (p. 19): "The low center at the height of Mount Washington sometimes lags behind the low center at the surface of the earth apparently as much as two hundred miles."

The Rev. Clement Ley, of England, in a paper published in the Quarterly Journal of the Meteorological Society for July, 1879, adopts this statement advanced by Professor Loomis in support of his hypothesis that the storm center lags behind at elevations as shown by his own observations of cirrus clouds. Mr. Strachan in discussing this point as brought out by Mr. Ley has shown that, taking the average velocity of a storm center as twenty miles per hour, and a retardation of one hour per each thousand feet of ascent, the axis of the storm center must be inclined 89°·5 to the vertical, or, in other words, lie almost parallel with the earth's surface.

Professor Ferrel, in his paper, "Meteorological Researches for the use of the Coast Pilot, Part II," has advanced the theory that a storm center lags behind at the earth's surface, and, speaking of the apparent contradiction of the researches of Professor Loomis, says: "These results of Professor Loomis show too much for this hypothesis" [that the axis of a "low" is retarded at elevations], "for they show that there is a similar retardation of just about the same amount in the times of the maxima and minima of the diurnal changes of barometric pressure at the summits of mountains, and we cannot reasonably explain this by means of cyclones with reclining axes. When we shall have a satisfactory explanation of this retardation in this latter case, we shall probably have one in the other."

In discussing the probable cause for this retardation, the influence of high winds and of variations in temperature will be considered, and the theory that it can be caused by the lagging of the axis of a "low" abandoned as untenable, for this, if for no other reason, that, unless a secondary force acts, it is exceedingly difficult to conceive how any fall of pressure at the summit of a mountain would not make itself felt at the same time at the base through the superincumbent atmosphere.

If a strong current of air can produce a depression of the mercurial column of a barometer, as has been shown by using a blower for the air current and an air-tight receiver for the barometer (a condition of things, however, which, it should be be borne in mind, can never occur in nature), then why may not the high wind which nearly always accompanies a storm cause a fall in air-pressure, which shall continue as long as the wind is strong at the summit and some time after the storm center has passed the base?

It is exceedingly desirable that special experiments be made under natural conditions, directly testing the influence or non-influence of high winds on the indications of the barometer. In the absence of such experiments we may indirectly ascertain such influence by comparing observations made at the same moment of time at the summit and base of mountains. The materials for such comparison exist in manuscript copies of hourly observations at the base and summit of Mount Washington during May, 1872, which I have been allowed to consult through the kindness of the Chief Signal Officer of the Army; and in the hourly observations of May and June, 1873, made at the same stations. The latter have been published in the annual report of the Chief Signal Officer for 1873. These stations lie three miles apart in a horizontal direction, and hence we may compute with a near approach to accuracy, from the observations for pressure and temperature, the difference of level between them, by the use of Guyot's hypsometric formula.

In order to show that varying temperature does not appreciably affect the relative results of such computation, the following comparisons are given.

At a recent meeting of the Washington Philosophical Society, Professor G. K. Gilbert, of the United States Geological Survey, gave a method for determining differences of elevation where air-pressures are observed at three or more neighboring stations having different heights, which may be outlined as follows: Let P, P′ and P″ represent the observed air-pressure at stations A, B and C, then will the formula,

$$H = C(\log P - \log P')f(t)f(O)f(m) \qquad (1)$$

express the difference in height between A and B, and

$$H' = C(\log P - \log P'')f(t)f(O)f(m) \qquad (2)$$

the same for A and C; dividing (1) by (2) we have

$$\frac{H}{H'} = \frac{\log P - \log P'}{\log P - \log P''} + D:$$

if now we have either H or H′ given, we may compute the other, without referring to temperature, latitude or moisture.

During the month of June, 1873, hourly observations were made at the base and summit of Mount Washington and at two intermediate stations. Considering forty-three cases taken at random, I find twenty-three of them in which the mean height of the summit above the base is too small, by Gilbert's and Guyot's formulæ, forty-six and fifty-five feet respectively; while the remaining twenty cases give a value too great by fifty-one and forty-eight feet. We may therefore, in comparing relative heights, neglect the effect of varying temperature as introduced by computations with Guyot's formula.

If the wind affects the pressure directly, we would expect that the computed difference of level would be the same as the true difference when there was no wind, and would gradually increase as the wind increased, unless there were some causes beside pressure, temperature and wind affecting the computation. I have grouped these computed differences in elevation according to the force of the wind, as may be seen in Table I.

In the following table, for May, 1872, all winds under ten and above forty miles per hour are included, and in May, 1873, all the cases except a few which were omitted because of serious errors in the observations. The table shows this remarkable peculiarity, that, though with winds above sixty-one miles per hour, the mean computed difference in height is too great by sixty-six feet: with winds under ten the mean difference is too small by thirty-five feet. We conclude, then, that some other

cause must produce the results or must act in conjunction with the wind. Taking winds above sixty-one miles per hour I have found ten cases in which the height was too small by about fifteen feet; also, a great number of cases in which though the wind continued strong from the same direction, yet the computed height continually became less, showing that the wind does not produce a direct effect upon the indications of the barometer.

TABLE I.

Mean amount to be added to the true difference in height between the summit and base of Mount Washington to give the computed difference.

	Wind force in miles per hour.													
	0 to 10.		11 to 20.		21 to 30.		31 to 40.		41 to 50.		51 to 60.		Above 61.	
Date.	Cases.	Amt.	C.	A.	C.	A.	C.	A.	C.	A.	C.	A.	C.	A.
May, 1872.	77	−27′·1	25	−18′·6	30	−3′·1	43	+13′·8	65	+10′·5	32	+33′·9	50	+51′·4
May, 1873.	104	−43·5	134	−22·0	183	+4·1	135	+15·6	99	+34·9	61	+52·4	27	+80·1

On projecting the curves of pressure in connection with the computed elevations, we find that there is a striking uniformity in the occurrence of small and large differences of elevation with the maxima and minima of pressure, the least coinciding with high pressure and the greatest with low. Grouping a second time, then, with respect to the maxima and minima of pressure we have Table II.

TABLE II.

Mean amounts to be added to the true difference in height between the summit and base of Mount Washington to obtain the computed difference.

		Maxima of Pressure.		Minima of Pressure.	
Date.	Locality.	Cases.	Amount.	Cases.	Amount.
May, 1872	Mt. W. and base	81	−32′·5	70	+ 57′·4
May, 1873	Mt. W. and base	102	−61·6	137	+ 67·3
Jan., Feb., Mar., Oct., Nov., Dec., 1880	Mt. W. and mean of B. and P.	119	−29·1	120	+127·0

In the above table, as the first two horizontal rows of figures apply only to observations for the month of May, I have added a third set of figures for the summit of Mount Washington compared with the mean of Burlington and Portland as the base, and computed for observations taken at 7 A. M., 3 P. M. and 11 P. M., Washington time, during Jan., Feb., Mar., Oct., Nov. and Dec., 1880. Burlington and Portland, near sea-level,

lie on opposite sides of Mount Washington and at distances altogether too great to give the best results; if, however, we take the mean of the two we shall obtain an approximate value for the base of the mountain.

It is evident from Table II that during the prevalence of relatively high pressure, elevations, computed barometrically, will in general be too small, and, on the other hand, when the pressure is low the computed heights will be too great. This fact also explains the coincidence of too great computed heights with high winds, for the reason that the highest winds always occur with relatively low pressure; on the contrary, when the wind is light the pressure is generally high. We may conclude, then, that the same cause which produces the maxima and minima of pressure, controls also the different values of computed elevations, and that high winds are rather an accompaniment than a direct cause of the same variations.

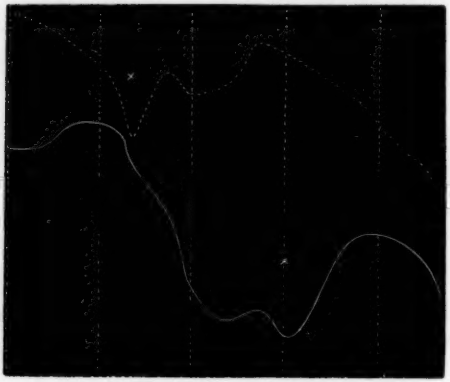
May not the apparent lagging of the axes of "highs" and "lows" be due to the effect of varying temperature. The general tendency of high temperature being to expand the air and force it from the lower levels, above the summits of mountains, and of low temperature to produce an opposite effect, we should expect at elevated stations high pressure with high temperature and low pressure with low temperature. This principle is well illustrated by the Signal Service observations of pressures on Mount Washington and Pike's Peak. In January the mean pressures are 23″·393 and 17″·512 at the two points respectively, while in July they are 23″·894 and 18″·078. The same results would follow the accidental fluctuations, whenever there might be a steady, gradual rise or fall in temperature for a sufficient period of time. When a "low" has passed a station at sea-level, the temperature frequently falls steadily with a west wind and the result is a contraction of the air, which causes its withdrawal from the upper atmosphere and a further fall in pressure there. This process will continue until the fall caused by the low temperature is counterbalanced by the rise due to the advancing high. The reverse of this may take place at the passing of a "high." In order to ascertain the influence of varying temperature as suggested above, I have projected the pressure curves for a singular case at Pike's Peak, in which the minimum occurred forty hours earlier at Denver than at Pike's Peak; also, for a case in which the minimum for Burlington and Portland occurred twenty-four hours earlier than on Mount Washington.

Referring to the curves and the temperatures at Pike's Peak, we see that on November fourteenth, at the 7 A. M. observation, the mean temperature of the air column was comparatively low and the pressure at a maximum at Denver; the temperature

gradually increased to three P. M. of the fifteenth, and though there was a steady fall in pressure at Denver, yet it rose on Pike's Peak until seven A. M. of the fifteenth; from this point

Retardation of the minimum of pressure at Pike's Peak, Nov. 15 *and* 16, 1880.

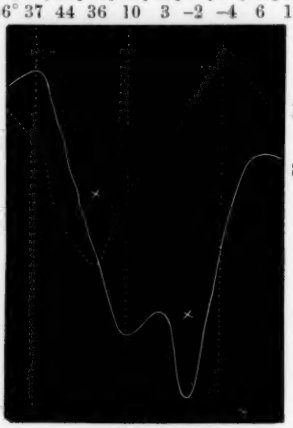
$14_1 14_2 14_3$ $15_1 15_2 15_3$ $16_1 16_2 16_3$ $17_1 17_2 17_3$ $18_1 18_2 18_3$ Date.
6° 20 19 22 34 16 6 1 −6 −20 −10 −12 −6 7 6 Mean Temp.



Curves of Air-pressure.

Mt. Washington and mean of Burlington and Portland, Nov., 1880.

17_3 18_1 18_2 18_3 19_1 19_2 19_3 20_1 20_2 20_3 Date.
36° 37 44 36 10 3 −2 −4 6 13 Mean Temp.



Curves of Air-pressure.

the pressure rose steadily at Denver under the influence of an extraordinary cold wave; this same cold wave reduced the pressure on Pike's Peak, which did not reach its minimum till seven A. M. of the seventeenth, or forty hours later than at

Denver. On a comparison of the curves at Mount Washington, we see another illustration of the same nature.

If now we apply the same reasoning to the maxima and minima of the diurnal range of pressure, at the base and summit of mountains, we shall find that it affords a satisfactory explanation of the retardation at the summits. For the purpose of comparison I have taken the hourly observations at Mount Washington and base in May, 1872 and '73, and in June, 1873; also, a manuscript copy of hourly observations made by the observers of the United States Signal Service at Pike's Peak and Colorado Springs during August and September, 1874. The means for each hour have been embodied in Table III. The difference in level between the base and

TABLE III.

Diurnal range of air pressure, and the mean temperature of the air column at Mount Washington and Pike's Peak.

Hour of day.	May, 1872.			May and June, 1873.			Aug. and Sept., 1874.		
	Air pressure.		Mean T.	Air pressure.		Mean T.	Air pressure.		Mean T.
	Mt. W.	Base.	Air col.	Mt. W.	Base.	Air col.	Pike's P.	C. Sp'gs.	Air col.
1 A.M.				23″·760	27″·165	37°·2			
2				·757	·161	38·8			
3				·751	·158	38·4			
4				·746	·160	38·0			
5				·753	·165	37·8			
6	23″·675*	27″·098†	36°·2	·756	·173	38·6	17″·982	24″·201	47°·1
7	·670	·100	37·4	·762	·180	40·1	17·988	·202	51·8
8	·671	·101	38·7	·767	·181	42·2	17·995	·204	55·0
9	·674	·103	39·6	·773	·181	44·0	18·002	·205	58·0
10	·683	·098	40·6	·781	·180	45·1	18·009	·198	60·4
11	·689	·096	41·9	·784	·176	46·6	18·009	·188	62·8
Noon	·690	·093	42·6	·787	·169	48·0	18·007	·176	64·3
1 P.M.	·690	·085	43·3	·785	·163	49·0	18·005	·164	65·0
2	·687	·081	43·6	·783	·157	49·0	18·002	·150	64·4
3	·689	·082	44·0	·777	·152	48·8	17·998	·144	63·3
4	·688	·078	43·8	·775	·150	48·6	17·994	·142	61·2
5	·683	·066	44·0	·771	·150	47·6	17·992	·146	60·1
6	·691	·084	43·2	·771	·155	46·2	17·992	·152	58·2
7				·772	·163	44·3	17·997	·165	55·5
8				·773	·166	42·4	18·001	·180	53·5
9	·680	·102	38·5	·779	·171	41·7	18·003	·188	52·3
10				·775	·171	41·0			
11				·770	·171	40·2			
Midn.	·676	·095	37·4	·765	·167	39·8			

	Mt. W.* Uncorrected Barometer.	Att. Ther.	Base. Att. Ther.
6 A.	23″·730	55°·1	50·4
7	·730	57·8	49·5
8	·739	60·7	49·4
9	·750	64·6	49·7
10	·762	65·7	49·8

summit of Mount Washington is 3590 feet, and between Pike's Peak and Colorado Springs it is 8100 feet.

The figures in this table, in the fifth and sixth columns, were taken from Professor Loomis' tenth paper; the remaining columns I have computed from the original observations. Unfortunately the hourly observations of 1872 and '74 were continued only during the day hours; they, however, give satisfactory results for the principal morning maximum and afternoon minimum. A marked peculiarity will be noticed in column two in the means of the observations for seven and eight A. M., namely: a steady fall during these hours. This is due not to natural causes at all, but to the fact that at the midnight observation the barometer was pushed into its case and locked up. In this position the temperature indicated by the attached thermometer was lower than that of the room, consequently, by the time of the second observation the next morning, after the barometer had been suspended for an hour in the room, the temperature of the air in the room would be indicated by the attached thermometer, while the mercury and scale of the barometer would change their temperature much more slowly, and the correction for temperature being too great, the readings of the barometer were made too small, until the different parts of the instrument had attained a common temperature. The attached thermometer at the base indicates that the observations there were not made in the living room, or else that the fire in the room was very low. (See note at foot of Table III.) It will be noticed that in each case after the morning maximum of pressure was reached at the base, the mean temperature of the air column was rising, and in consequence, the pressure did not begin to fall at the summit till some time had elapsed. For the afternoon minimum a reverse of the conditions and effects is noticeable. One interesting circumstance should not be overlooked: while at Mount Washington the lagging in the morning was three hours and a half for 3590 feet, or one hour to each thousand feet, at Pike's Peak it was only two hours for 8100 feet, or one hour to 4000 feet. This is due in part to the fact that after the morning maximum the fall in pressure was much more rapid at the lower western station than at the eastern, and this counterbalanced the rise in pressure at the summit due to the increase of temperature.

Again, we see that in the single case we have of observations through the night, there is little or no retardation in the morning minimum and night maximum. This is precisely what we might conclude from the fact that during this time the temperature changes only slightly, though little stress can be laid upon this because the oscillation is correspondingly small.

Another cause for a portion of the retardation may be considered to be the sluggishness in action of the summit barometers, or again, to be the slowness with which the air at the maxima and minima of pressure would enter and leave the room where the observations were made, undoubtedly less accessible to the atmosphere at the summit than at the base. These last two causes, however, would be of little consequence in producing the results noted.

May we not be enabled, by a sufficient number of carefully conducted observations, extending over a year, at the base and along the side of Pike's Peak or some other isolated mountain, to determine the cause of the diurnal range of air pressure, which has been characterized as the most persistent of all meteorological phenomena and never as yet satisfactorily accounted for? If the above discussion assigning varying temperature as the cause of the apparent retardation of the axis of a "low" at high stations prove satisfactory, it will also show the great difficulty which inevitably attends any attempt made in comparing pressures observed at elevations above the earth with those observed at the same time at sea-level.

Acknowledgment is due to Professor Abbe for suggestions in the final construction of this paper.

ART. XV.—*On the General Principles of the Nomenclature of the Massive Crystalline Rocks;* by A. WENDELL JACKSON.

INTRODUCTION.

THE objects of the present paper are, first, to suggest the complete separation of rock-nomenclature from rock-classification, and, second, to investigate and to establish, as far as may be, the principles upon which any system of nomenclature for the massive crystalline rocks should be based. Throughout my paper the term "nomenclature" is to be understood as referring to the names of the rocks themselves and not of the larger groups recognized in rock-classifications.

The existing confusion in rock-nomenclature is due to several causes, not the least of which is the admission of classification as a controlling feature in nomenclature, and the tendency seems to be more and more in this direction. This principle is even directly advocated in a recent able work (Dutton, "Geology of the High Plateaus of Utah," Chap. iv). Its general recognition can only result disastrously to the science. The true function of present systems of classification is to express relations between the objects classified. The true function of a system of nomenclature is to furnish each of these objects with

a name that shall be as little subject to change as possible, for change produces confusion.

Classifications are constantly changing and if our rock-names are made dependent upon them, an element of confusion in nomenclature is introduced that is both undesirable and unnecessary. Our only remedy lies in a complete divorce of nomenclature from classification such as has long been recognized in zoology, botany and mineralogy.*

In the following pages I wish to discuss the principles of nomenclature as considered thus entirely distinct from classification; it goes without saying that the chemical, mineralogical, and geological considerations which I discuss with reference to the irbearing on *nomenclature* are entirely open to a re-discussion with reference to their bearing on *classification;* and one may arrive at diametrically opposite conclusions as to their value for classification from my own as to their value for nomenclature without in the least invalidating the results reached in the present paper.

In the first chapter I state and explain the principles which I conceive should guide us in the formation of rock-names and I then examine the considerations at our disposal to determine how far they may be used in conformity with the principles laid down. In the second chapter I explain why I hold it to be inexpedient to formulate a system of names in accordance with the principles suggested and give reasons for adopting provisionally a different system of names.

Chapter I.

I conceive the fundamental principles to be borne constantly in mind in the formation of a system of nomenclature to be three: I. *Uniformity*, II. *Stability*, III. *Adaptability*. I shall examine each of these and indicate its bearing upon the question at issue.

I. *Uniformity*. No one will for a moment dispute the desirability of having but *one* name for each rock. There is something of a tendency toward the formation of "schools" in this respect. That is all wrong however; all geologists should call the same rock by the same name. Zoologists, botanists and mineralogists recognize this principle in their respective sciences. Indeed it is so evident that one who will dispute it is not to be reasoned with.

II. *Stability*. By a "stable" nomenclature, I mean one that is not subject to change. A rock that has once received a definite name should continue to be known by it. This principle is second in importance only to the first. If *all* geologists would agree to change any given name, there would of course

* For a more extended discussion of this topic see the reprint of this paper in the Proceedings of the California Academy of Sciences. Volume for 1882.

be no objection to the change; uniformity would still be preserved. This principle as well as the first has been frequently violated in petrography.

III. *Adaptability.* In considering this principle it is desirable to bear in mind the peculiar nature of a rock as distinguished from a mineral. A mineral is a definite homogeneous chemical compound and one usually has no difficulty in recognizing it and applying at once the name that has been agreed upon for that particular compound. But a rock is usually a mechanical mixture of two or more minerals, admitting considerable variation in the relative amounts of the different constituents, remaining in the estimation of the geologist substantially the same rock. By the "adaptability" of a name I mean that it should adapt itself to this variation. A change in the relative amount of the constituents within certain not all-too-broad limits should not necessitate the use of a new name.

It is here that I conceive some may take issue with me as to the practicability of upholding this principle. It will be contended that rocks grade off into each other by insensible transitions and that the *transitional forms* are just as common as those we may choose to set up as *typical forms;* that a name consequently cannot be made to adapt itself to this indefinitely shifting mixture of minerals. The answer to this has been so finely stated by Rosenbusch (Massige Gesteine, p. 25) that I cannot refrain from quoting it in full, premising that I have fully experienced the truth of his statement.

He says: "In the same way that insensible transitions from one rock to another are brought about by change in the mineralogical composition, so also is it true that similar transitions are sometimes the results of a change in texture. I believe myself justified in the statement that every thoughtful petrographer will have passed through three stages of development in his scientific views, with increasing experience in this direction, stages which possess more than a personal interest, from the fact that they are likewise recognizable as three stages in the development of the science itself. In the first of these stages, one is a fanatic for a *simple system;* one recognizes a series of well characterized types and no transitional forms. In the second stage, the conviction presses itself gradually upon the mind that nature cannot be fitted into so exact and rigid a frame; the well characterized rock-types become fewer and fewer, the transitional forms more and more numerous. One turns away from the rigid system and becomes an inspired advocate of the uninterrupted series, of the gradual development of one rock from another, of the insensible transitions which bind together and totally obliterate the types of the first stage. Finally, in the last stage, one begins gradually to discover cer-

tain fixed points in this vague and indefinite mass of transitional forms; or, to express myself petrographically, in the formless magma of our views crystallization-centra begin to appear, about which homogeneous material begins to deposit itself concentrically with gradually decreasing density as we recede from the center. Thus arise rock-groups whose central types are well-defined and distinctly separated from one another, while their peripheries tangent and in many ways coalesce. One now returns to the definite system which no longer possesses however the rigidity and dead immobility of the first period."

One who believes there is no such thing as a definite rock-type, finds himself in the second stage of his development and only needs a more extended range of experience to discover the rock-types and to view them in their true relations; and he will then perceive the true significance of this principle of adaptability and will recognize the importance of conceding to it a high degree of influence in the construction of rock-names.

Mr. Darwin has demonstrated the necessity of recognizing this principle in the organic natural sciences and the mineralogist has to bear it constantly in mind.

Were the necessity at all incumbent upon us to form a system of nomenclature for material that had reached a *perfect* natural classification, I should certainly add a fourth principle to the three thus suggested, namely, that a name should be constructed so as to suggest the relation of the rock to all allied rocks; in other words I would seek to construct a complete *systematic* nomenclature that should be an exact expression of the *systematic* classification.

As such a system has not yet and may never be reached, it is certainly unwise to allow this principle to enter largely into the determination of present nomenclature. It would at once conflict with the first two principles of uniformity and stability. To a slight extent it may perhaps safely be done; we shall consider such cases later.

Having thus laid down certain principles to guide us, I will now examine the considerations that might influence our selection of rock-names. In so far as these considerations are in accord with these principles, they will be endorsed; in so far as they are in conflict, they will be rejected.

I claim, first, as of fundamental importance that *facts*, and facts alone, should determine names and that *speculations*, *hypotheses* and *theories* should be entirely ignored. Facts are susceptible of demonstration, and a nomenclature founded upon them is sure to be uniform and stable, while speculations, hypotheses and theories admit of differences of opinion, and if our names are to be influenced in any way by them, we at once admit an element

of nonuniformity and instability. Although it is in a general way true that the theories of one generation become the facts of the next, it is equally true that *most* of the theories of one generation *fail* to become the facts of the next; and so long as they remain in the condition of a theory they should be totally ignored in forming rock-names. The value of a fact in this connection is to be measured by its generality; facts of local value should be reserved for local distinctions.

The kinds of facts which are more immediately accessible to the geologists are three: *chemical*, *geological* and *mineralogical*. I will indicate what I conceive to be the value of each class for the formation of names.

1. *Chemical.* Perhaps the most striking evidence of the impossibility of basing nomenclature upon chemical composition can be presented in the fact that during all of the years preceding the use of the microscope in petrographical investigation, chemistry was unable to discover distinctions that become evident with the first glance into the microscope. Rocks are not minerals and have not the same well-defined stochiometric composition that minerals have. On the contrary, rocks that are mineralogically identical show considerable variations in chemical composition, while the same bulk-analysis may be true for rocks of widely different mineralogical composition. It is to be observed from this that knowing the analysis one is not able to predict mineral composition while the converse is *not* true: if we know the mineral composition we can predict always qualitatively, and to an approximation quantitatively, the chemical composition. It would seem then that the fact of mineral composition is of a higher order of utility than the fact of chemical composition. The latter is a function of the former and a nomenclature based upon mineralogical facts will necessarily express sufficiently clearly the chemical facts, without the inconvenience of determining the chemical composition in each case before deciding what name shall be applied.

Again, it is held by some (Scheerer, Jahrbuch der Mineralogie etc., 1864, 385, and others) that the chemical composition of a rock is a fact of the first importance and should be made the leading element in classifying (and naming?) because it gives us a clear conception of the nature of the original fluid material out of which the rock crystallized; and that it is this original fluid material which constitutes the true "rock-type" about which all of our interest centers. Now, while I fully admit the value of this conception and appreciate the significance of the results to be gained by tracing different rocks back to a common magma, still I hold that a rock is something more than a certain chemical mixture, it is a chemical mixture *with a history that is equally interesting.* It has been subjected to various

physical conditions of temperature and pressure that have determined its rate of crystallization and that, equally with the original magma, have determined the precise minerals which we now find in the rock itself. It is true we do not yet know enough of the relations between varying conditions of temperature and pressure, and the resulting minerals to be able to reason backward from the minerals themselves to the conditions of which they are the (partial) expression; some progress has been made in this direction and certainly more is to be expected.

The mineral composition of a rock will give us a conception, then, not only of the original chemical composition of the magma from which it was derived. but also (particularly with *texture*, see p. 19) of the physical conditions which have prevailed during the process by which the rock acquired its present appearance; it suggests both chemical *and* physical conditions while the chemical analysis suggests only the former. Whence I conceive that the use of chemical facts in constructing rock-names is superfluous.

The inconvenience of making a quantitative chemical analysis before deciding the name of a rock is certainly not the least objectionable feature of the plan.

Geology must always remain more or less of a natural history science and the field geologist will always need a name that will convey some conception of the *appearance* of a rock, such as a mineralogical name will give him.

2. *Geological.* The geological facts that might be used in this connection are *form*, *age* and *origin*. *Form* has been used to some extent, but it is now so generally recognized that the same rock can occur as dyke, bed, sheet, stream, or stock, that it has properly ceased to have any significance so far as name is concerned. Not so, however, with *age*. The continental geologists divide the massive crystalline rocks into two classes which they call "older" or "younger" according as the rock was formed *before* or *after* the inception of the Tertiary period; and based upon this distinction, the same mineral aggregate is provided with two different names according as it is of Pre-Tertiary or Post-Cretaceous age. Thus I have held in my hand two specimens that could not be distinguished from each other even in thin sections under the microscope. Both contained quartz and sanidine porphyritically developed, the groundmass of both was crypto-crystalline throughout and yet the one was "quartz-porphyry" because Pre-Tertiary, and the other was "quartz-trachyte" because Post-Cretaceous. For the same reason melaphyr is separated from basalt, with which it is otherwise identical; hornblende porphyrite from hornblende

andesite; augite porphyrite from augite andesite; orthoclase porphyrite from trachyte; and diabase from dolerite.

Those who hold to this distinction do not indeed deny the frequent mineralogical identity of the rocks thus separated; but they hold that it is *desirable* to express thus the difference in age of the same mineral aggregate. With Prof. J. D. Dana* I must confess myself unable to see the value of this distinction for the nomenclature of the science. It appears to me much more rational to call a certain mineral aggregate "quartz porphyry" or "liparite" (use which name one will) and to recognize the fact that the rock thus named is sometimes of Silurian, sometimes of Carboniferous, sometimes of Permian, and sometimes of Post-Cretaceous age. We find no difficulty in mineralogy of uniting the quartz of the Pre-Tertiary quartz porphyry with the quartz of the Post-Cretaceous quartz-trachyte (or liparite, or rhyolite) under one species, in spite of the fact that geological periods separated the dates of their formation. One who would advocate their separation into two species would be properly ignored and yet the geologist makes a similar unnecessary and unphilosophical distinction daily.

Moreover those who insist upon this distinction are not consistent. They separate quartz porphyry from liparite, but they fail to separate the Paleozoic granite of the Harz Mts. from the Mesozoic granite of Cornwall; and they fail to separate certain Triassic quartz porphyries of Germany from Silurian quartz porphyries of England. In other words, the line is drawn between Mesozoic and Cenozoic but not betweeen Mesozoic and Paleozoic. The reason for this (Roth, Beiträge zur Petrographie der plutonischen Gesteine in den Abhandlungen der Akademie der Wissenschaften zu Berlin, 1869, p. 74) probably lies in the fact that the earlier geologists were led, through the study of the Mesozoic rocks of Central Europe, to the conception that a period of comparative quiet separated the widespread igneous activity of the Azoic and Paleozoic ages from what appeared to be a renewal of this activity in a different form in Tertiary and Post-Tertiary times. It appeared as though the nature as well as the products of this renewed activity were different. The pronounced absence of volcanoes, lava streams, and tufas seemed to indicate that Pre-Tertiary activity was *subterranean* while *recent* activity finds expression *at the surface* in the form of volcanoes, lava streams, tufa eruptions, and such tremendous sheet overflows as those of Utah, and of Washington Territory, Oregon, and Northern California.

The results of more recent and more widely extended researches do not seem to favor this idea. Paleozoic volcanoes, lava streams, and even tufas have been found and we should

* J. D. Dana. On some points in Lithology. This Journal, iii, p. 336.

certainly expect *à priori* that they would have been formed then as now; but in the tremendous lapse of time that has intervened they would necessarily have become either greatly modified in form by erosion or in appearance by subsequent metamorphism so as to become unrecognizable as such, or indeed entirely obliterated. It would require peculiarly favorable conditions to preserve a Paleozoic tufa in a recognizable condition to the present day. On the other hand, it is now conceived that granitic rocks are forming in the subterranean depths at the present time. The differences that do exist between so-called "older" and "newer" rocks may be due then not to their difference in age, but to the difference in the physical conditions of their crystallization, conditions which held equally in Paleozoic times and in Tertiary times, only the Paleozoic superficial formations have been mostly swept away and the deep seated rocks of that age exposed by the removal of thousands of feet of superincumbent strata while the superficial rocks of Post-Cretaceous times, the trachytes, andesites, phonolites, basalts, etc., are preserved to us because of their recent origin and the granitic rocks *now forming* are too deep-seated to be exposed for observation.

I would oppose then this distinction of rocks into "older" and "newer" (for purposes of nomenclature) first, because the original grounds upon which it was made seem no longer tenable; second, because such distinctions as exist *in fact* between older and younger rocks can be accurately expressed by a purely mineralogical nomenclature; third, because the distinction is not and cannot be consistently carried out between rocks of Mesozoic and Paleozoic age; fourth, because of its great inconvenience. It is not always possible to tell the age of a rock; in the meantime its name must be held in abeyance or at least subject to change. More than one instance will suggest itself to the specialist where the name of a rock supposed to be Pre-Tertiary has been changed when subsequent study in the field has proved the rock to be Tertiary or Post-Tertiary. This element of uncertainty plays sad havoc with rock-names and should be set aside entirely.

The third geological consideration to be noticed is *origin*. The first objection to be urged against its use in rock-naming is the fact that the whole subject of rock-genesis lies too much within the region of theory and even speculation; whence for reasons previously indicated, it should be disregarded in this connection. But even if we could determine for each rock its exact mode of formation, even if we could attach definite conceptions to such terms as "metamorphism," "aqueo-igneous fusion," etc., the objection similar to that urged against "age" would still remain, namely, rocks with the same mineralogical

composition would be provided with different names according as they were formed by one process or another. When actual differences in composition result from different modes of formation, such differences will find expression in a mineralogical nomenclature, and when no differences exist, it would seem far more rational to use but a single name. I would quote here, with additions of my own in italics, from Prof. J. D. Dana's Manual of Geology, 3d edit., p. 76. "Further, rocks, as objects in science, should be defined and named according to their kinds,—not according to the era of formation *nor the method of formation*,—since the same things are the same whenever made *and however made*." Fortunately there is no pronounced tendency to transgress in this direction.

3. *Mineralogical.* We come now to an examination of the mineralogical facts at our disposal, in the light of the three principles previously laid down. Under this head I propose to include both the *minerals* themselves and the manner in which they are combined together, in other words *rock-texture.*

First, with respect to the minerals themselves. They are susceptible of exact determination, there can be no differences of opinion as to what the essential constituents of a rock really are. The use of the microscope, the application of polarized light whereby the positions of optical planes can be accurately determined, Sorby's method for determining the indices of refraction of minerals in rock-sections, Thoulet's method of mechanically isolating rock constituents so that they may be quantitatively analysed if necessary, have all reduced the process of mineralogical determination of the constituents even of compact rocks to great exactness.

It may be conceived that the difficulty, rarely perhaps impossibility, of distinguishing between the different kinds of plagioclase * would interpose a serious obstacle in the way of a purely mineralogical nomenclature. But it should be remembered on the one hand that Descloizeaux' optical distinctions and Thoulet's method of mechanical isolation, together enable us, in most cases, to make the distinction with the greatest accuracy and, on the other hand, that the practical distinction between the different forms of plagioclase that occur as rock constituents is of secondary importance compared with the fundamental distinction between orthoclase and plagioclase, and this latter we have always been able to make with the greatest ease.

While it cannot be regarded as proved that albite and anorthite, the soda-plagioclase and lime-plagioclase, do not widely occur as rock constituents, still the results of investigations up to the present point in this direction. It is the intermediate

* I use the term "plagioclase" in its original sense, including the albite-anorthite series and excluding microcline.

members of the plagioclase group for which oligoclase and labradorite stand as types, that most commonly occur. No great element of uncertainty would be introduced into our nomenclature if we considered plagioclase a single mineral species, subject of course to variation in the amounts of soda, lime and silica, and in the future progress of the science, as the exact nature of the plagioclase for each individual case became determined, the name of this variety could easily be substituted for plagioclase in the name of the rock, or used as an adjective modifying the name, if a purely trivial one. We should have, e. g. "oligoclase-basalt" and "labradorite-basalt" in the place of "basalt" as now used. It must certainly be conceded (as indeed it is generally believed) that no weight can attach to the objection that the constituent minerals of a rock cannot be accurately determined.

Supposing, now, that our nomenclature is to be based upon purely mineralogical grounds, it becomes necessary to determine what minerals shall be utilized for this purpose. The mineral constituents of a rock have been divided into *primary* and *secondary* according as they are the product of immediate crystallization out of the original rock-magma before or at the final solidification of the rock; or are produced by changes in the rock subsequent to its final solidification brought about by atmospheric waters or local metamorphism. To these may be added such minerals and fragments as may have been mechanically enclosed during the process of eruption; I shall call such foreign minerals and fragments. With the fragments of course we have nothing to do.

Of these the secondary and foreign minerals should unquestionably be disregarded in naming the rock, as in fact has always been the practice. They are evidently purely adventitious and have nothing whatsoever to do with the rock as such. Their true nature can always be recognized and they consequently introduce no element of uncertainty to create confusion in names. There remain the primary constituents. Among these a certain liability to confusion may exist in the distinction between *essential* and *accessory* constituents. The accessory mineral of one rock is the essential mineral of another, and just where to draw the line is not always at once evident from the fact that an approximate quantitative estimate of the mineral has to be made. It is here that practice only can render skillful. From the nature of the case it can never be otherwise, and serious embarrassment is not to be feared from this source. It is to be presumed that geologists are to be educated and the ability to make such distinctions must be learned. He who runs cannot expect to read. On the other hand, it must be observed that mistakes arising from this

source are liable only about the *peripheries* of the rock-groups, to take up Rozenbusch's figure once more; and the alternative is to place the rock in a closely allied group where the mistake, if it be one, can do the minimum of harm.

I have shown thus far that *uniformity* and *stability* would result from a purely mineralogical nomenclature; let us now examine the *adaptibility* of such names. I have already called attention to the variation within certain limits in the relative proportion of the essential constituent minerals of any rock. It is not worth while to express this variation in our nomenclature. In fact it is so irregular, so completely beyond the control of any known or conceivable general law that it would be altogether impossible to express it if we would. Names founded upon mineral composition would ignore this variation so long as it took place within the limits assigned for *essential* constituents. Such names would therefore completely adapt themselves to the natural conditions. They would have moreover even a higher order of adaptability than this. I have quoted Rosenbusch on the subject of rock-groups wherein he asserts the substantial unity of rock-types but of rock-types that are themselves united by transitions due to the gradual replacement of one or more constituents by other minerals, or by a gradual change of texture. Such names can be made to adapt themselves most perfectly to the expression of the relations between the different members of a rock-group and between each and the central type. *To this limited extent* the attempt could safely be made to be *systematic* in our nomenclature; for the relations between the closely allied rocks of each group both chemically and geologically as well as mineralogically are too evident ever to become questioned.

To illustrate with the granite group, Rosenbusch (Massige Gesteine, p. 18) following and developing the suggestion of Gustav Rose has divided and named the group in accordance with these principles. Quartz, orthoclase, and plagioclase are present in every granite; associated with these are muscovite, biotite and hornblende, sometimes one, sometimes two. When muscovite alone is present we have muscovite granite; when biotite alone, biotite-granite (or granitite); when hornblende alone, hornblende granite, when muscovite and biotite together, granite (in strict sense); when biotite and hornblende, biotite hornblende granite, or, more conveniently, hornblende granitite. Muscovite and hornblende do not occur simultaneously with the type constituents, but if they did it would be a simple matter to make a name for the rock.

The basalt group furnishes another good instance. From basalt as a central type (plagioclase, augite, olivine) we pass to nephelite basalt (−plagioclase + nephelite), or to leucite basalt

(−plagioclase + leucite), or to diallage basalt (−augite + diallage.)

In this way then our nomenclature could easily adapt itself to the special circumstances of each case. If it were considered desirable, purely trivial names could be given to each of these closely allied rocks; but my own preference would be for descriptive names so long as they could be formed by the use of a single mineral name as a modifier, such as "biotite granite," "leucite basalt," and for the reduction in number of the purely trivial names to a minimum.

In addition to the mineral constituents, many rocks contain more or less of the solidified magma-residuum that was not taken up by the crystalline elements before molecular freedom of motion was stopped by the final solidification of the rock. This varies in quantity all the way from 100 per cent (as in obsidian) down to 0 per cent as in granite. This evidently can not be disregarded; but the part it should play can be better discussed perhaps under the head of *texture.*

Rock Texture.—From the earliest times texture has been utilized for the purpose of naming rocks, and the desirability of continuing its use particularly for purposes of descriptive petrography is evident. It is none the less so from the fact that it is the expression, so far as we yet understand it even more perfectly than the minerals themselves, of the prevailing physical conditions during the process of cooling and solidification.

Can rock-texture be used in forming a nomenclature, without danger to our fundamental principles? Does it present itself in such forms as to be accurately and universally recognized? The accuracy with which Rosenbusch has defined (l. c. p. 70 et seq.) certain important forms of macro-, and micro-texture renders possible an affirmative answer to these two queries. I will reproduce his definitions. Ground-mass is the term applied to the compact portion of a rock as distinguished from the large, distinctly visible, disseminated crystals. It is entirely a macroscopic conception. Under the microscope this ground-mass may be developed entirely crystalline, i. e. anisotropic; or as a mixture of crystals and isotropic material called the *base;* or entirely isotropic. If entirely anisotropic, it may be either micro-crystalline, i. e. made up entirely of distinct crystalline granules; or cryptocrystalline, i. e. consisting of anisotropic material throughout but with the crystalline individuals altogether indistinguishable from one another. No sharp distinction can be drawn between these two; they pass by insensible transitions into each other.

If the ground-mass contains isotropic material, this material may be developed as a *microfelsitic base,* i. e. showing no polar-

ization and no glass, but consisting entirely of granules, scales and thread-like forms; or as a *glassy base*, i. e. a glass with products of devitrification; or as a pure *glass* without any of these devitrification products.

I would not pretend to base any distinction in nomenclature upon differences between these three latter forms of ground-mass. They are exceedingly interesting and important as showing at precisely what stage molecular freedom of motion was arrested in the cooling magma; or if one will, the precise stage of subsequent molecular rearrangement in the "older" rocks. But the distinction cannot invariably be sharply drawn between them.

One can however at once distinguish sharply between the *presence* or *absence* of isotropic material in the ground-mass, and upon this a distinction in nomenclature can safely be based. Bearing in mind the terms thus defined, I will illustrate the use that can safely be made of rock-texture, by applying it to the series of rocks having obsidian at one extreme and granite at the other, including thus the liparites and quartz porphyries.

If the rock is to the unaided eye distinctly and completely granular, it would be "granite." If some of the constituents were developed in larger crystals than the rest it could be indicated by the term "porphyritic granite." If the size of the granules diminished until the rock became compact and the texture were micro-crystalline or crypto-crystalline, it could be called with Rosenbusch "microgranite," and if porphyritic, "porphyritic microgranite." If the ground-mass contained isotropic material it would be desirable to distinguish between two cases; first, where the crystalline portion is in excess (the "quartz porphyries"), second, where the isotropic portion is in excess ("the obsidian porphyries.") I would call the first kind "quartz porphyry" whether the base were microfelsitic, glassy or pure glass. Or the trivial name "liparite" could be used, with the understanding however that the name should always indicate the presence of base in the ground-mass with disseminated crystals in excess. The "obsidian porphyries" could be distinguished, if desirable, as "microlitic obsidian porphyry," "felsitic obsidian, porphyry" and "glassy obsidian porphyry" according as the base showed a microlitic differentiation (as in the "younger" pitchstones), a felsitic devitrification (as in the "older" pitchstones) or remained in a purely glassy state. Finally when no recognizable crystalline minerals were present, the term "obsidian" could be used, and, if desirable, we could also distinguish "microlitic obsidian" (trachyte pitchstone), "felsitic obsidian" (felsite pitchstone) and "glassy obsidian."

Subjoined is a table of the terms I have thus constructed,

opposite to each the term Rosenbusch has applied to the same mineralogical and textural aggregate.

Jackson.		*Rosenbusch.*
Granite,		Granite. Granitic liparite (in part).
Porphyritic granite,		Porphyritic granite.
Microgranite,		Microgranite. Felsite fels.
Porphyritic microgranite,		Quartz porphyry. Granitic liparite (in part).
Quartz porphyry,		Liparite. Felsophyr. Vitrophyr (in part).
Obsidian porphyry	microlitic,	Trachytic pitchstone porphyry.
	felsitic,	Felsitic pitchstone porphyry. Vitrophyr (in part).
	glassy,	Obsidian porphyry.
Obsidian	microlitic,	Trachytic pitchstone.
	felsitic,	Felsitic pitchstone.
	glassy,	Obsidian.

In these names which I have thus suggested, the much abused term "porphyry" obtains a perfectly definite meaning; it indicates always the presence of determinable crystalline constituents and of a base, which is of course always isotropic. The term "porphyritic" would be used only in the macroscopic sense. These names are as much as possible descriptive; trivial names could be substituted throughout but, as I have said before, I think such names should never be coined where a convenient descriptive one will answer the same purpose. It will be observed that, following the principles previously laid down every possible textural aggregate in the series has received a name, that each has received but *one* name, that, consequently, the same name is never applied to aggregates that are objectively distinct, and finally that they are based upon distinctions about which there can be no differences of opinion.

In a similar manner these textural distinctions could be applied to every similar rock-series and perfect uniformity and consistency established throughout.

I have discussed in the present chapter only the most general considerations. Even if the principle of a strictly mineralogical nomenclature were accepted, the exact part which the different essential rock-constituents would play therein would still remain to be determined. The discussion of this question is premature, however, until the more fundamental proposition is recognized.

It seems to me clear that a rock-nomenclature founded upon purely chemical or geological or upon mixed chemical or geological principles either with each other, or with mineralogical principles, necessarily *must* lead, as it certainly *has* led, to much

confusion in the science; and that we can only hope for a satisfactory, uniform and consistent nomenclature when based upon mineralogical grounds alone. It is only thus that an exact system of names can arise that can give worthy expression to the exact work that is now being done in this department of geology. Until some such uniform system is established we must continue to waste our energies in struggling to understand one another when the real difficulty is not with the *facts* but with the *terms* in which we express them. The energies expended in this effort would be far more profitably utilized in advancing our knowledge of the facts themselves.

Justin Roth says that Petrography as a descriptive science loses all significance.* True, but it must be remembered that the purely *descriptive* stage of a science must always precede the *scientific* stage; and that the latter stage is only possible after the former has become *exact*. Whatever conduces to exactness of expression in descriptive petrography will add greatly to the *scientific* usefulness of this branch of geology in helping to solve many of the profoundest problems that engage the attention of the geological thinker of the present day.

Chapter II.

It will be observed that I refrain from any attempt to frame a system of names in accordance with the principles I have laid down. I do this because I think that no new name should be introduced into a science unless it is tolerably sure of being accepted in the sense in which it is proposed. Names are used to promote *clearness* and not *confusion* and unless this end can be attained it is better to refrain entirely from their introduction. The reformation of the entire nomenclature of a science is a task that should be attempted only by one who has gained authority by long years of special work, by one who is universally recognized as fitted for the undertaking. There are but one or two men living who could hope to succeed.

All that can be hoped for under the circumstances is that every writer should use the *same* nomenclature; *uniformity*, even if based upon principles that all will not accept as valid, is of the first importance. It is better that each should forego the luxury of insisting upon individual idiosyncracies, which in the majority of cases can never hope to become currently accepted than that by so doing he should add to the existing confusion. Just as strongly as Capt. Dutton† would insist upon

* Gest. Anal., 1873, p. 90. Die Petrographie, welche nicht mit den beschreibenden Zweigen der Naturwissenschaft in einer Reihe gestellt werden kann, gewinnt nur durch die Unterordnung unter die geologische Forschung ihre Bedeutung.

† Geol. of High Plateaus, p. 85.

the duty of each to express his ideas in the form of a classification, just so strongly would I insist upon the duty of each to refrain from using a name in a different sense from that in which it was originally proposed, or from that which is already current.

In selecting a nomenclature, that one should be chosen which is embodied in a convenient form accessible to all, which is freest from inconsistencies, and which has already attained the widest recognition. No American author, nor English, nor French can pretend to claim that his names are authoritative even in his own country. Germany if not the mother is at least the foster-mother of petrography and the literature of every other country is small compared with her own. In Germany two men—Zirkel and Rosenbusch—have won the highest recognition for their contributions to this branch of geology. Zirkel's "Lehrbuch der Petrographie" (1866) while still the best for field petrography is already too old to represent the present condition of the science. Of more recent works, Zirkel's "Mikroskopische Beschaffenheit der Mineralien und Gesteine" (1873) and Rosenbusch's "Mikroskopische Physiographie der massigen Gesteine" (1877) are most worthy of being cited. The latter is later and goes more systematically over the entire field. I think Rosenbusch's names are better chosen and capable of better defense than Zirkel's, where they differ. I shall follow Rosenbusch in the series of papers which I propose to present to the Academy of Sciences of California on the rocks of the Pacific coast.

Summary.

Permanence of rock-names is desirable; hence names should not be dependent in any manner upon the system of rock-classification, for classifications change.

The names of rocks should be *uniform*, i. e., used in the same sense by all geologists; they should be *stable*, i. e., not subject to change; they should be *adaptable*, i. e., to the somewhat variable nature of each rock.

In forming rock-names both *facts* and *theories* offer themselves as determining elements. The latter should be rejected as they admit of honest differences of opinion. Of facts, we have chemical, geological and mineralogical at our disposal.

Both chemical and geological facts should be rejected in determining rock-names, because mineralogical (and textural) differences among massive crystalline rocks can be adequately expressed by a purely mineralogical (and textural) nomenclature, and where such differences do not exist, it is undesirable to have names based upon geological or slight chemical differences.

It is only by the adoption of names based upon purely mineralogical differences that we can hope to obtain a nomenclature that shall conform to the three fundamental principles of Uniformity, Stability, and Adaptability.

As it is not to be hoped that a sweeping reform in petrographical nomenclature can be carried out at once, the good of the science requires that at least the first and most important of these principles, viz: Uniformity, should be recognized and in conformity therewith that all should use some one published and easily accessible system of names until a *better* complete system of names can be offered with some chance that it may be generally adopted.

In the opinion of the writer, the nomenclature of Rosenbusch as recorded in his "Massige Gesteine, 1877," is the most widely recognized and the best now accessible.

University of Cal., Berkeley.

ART. XVI.—*Communications from the U. S. Geol. Survey, Rocky Mountain Division.* I. *On the Minerals, mainly Zeolites, occurring in the basalt of Table Mountain, near Golden, Colorado;* by WHITMAN CROSS and W. F. HILLEBRAND.

(Continued from page 458, vol. xxiii.)

4. APOPHYLLITE.

THIS mineral occurs in well-developed crystals of prismatic habitus, with *i-i* (∞P∞) and 1(P) predominating, while 0(0P) is in most cases quite subordinate, or wanting entirely. The larger crystals, which are occasionally half an inch in diameter, are often of a greenish tinge, sometimes quite pronounced, and possess more or less uneven surfaces produced by a repetition of the crystal faces, so that the termination is made up of a large number of small pyramids.

The prismatic surfaces are roughened by depressions or elevations, bounded by prism and pyramid planes. This feature is very prominent in all large crystals. The smaller ones are in contrast sharp and clear, with smooth, brilliant faces. Especially noticeable on these small and clear crystals, though not peculiar to them, is a replacement of the pole edge of the pyramid, by a small reëntering angle formed by pyramid faces. This angle is nowhere prominent, yet may be easily identified on all clear crystals both large and small. No corresponding irregularity of any kind could be detected on the dimetric prism of these crystals.

As a rule, the largest crystals occur in the small cavities, and their growth has been more or less hemmed by the walls,

while the more perfect crystals are present in the large cavities, and are usually small and numerous.

In time of deposition apophyllite follows analcite. The optical properties of this apophyllite are noteworthy in so much as they seem to indicate very clearly the cause of the anomalous action, so often noticed in that mineral. No hypothesis of a complicated twin structure, such as that of Rumpf,* to cite the most recent, as well as the clearest and most consistent, can explain the phenomena observed in this apophyllite. While an extended description of the observed anomalies is impossible in this notice, the chief features will be given.

If from a small, clear crystal from Table Mountain a section be taken parallel to the base (0) and so situated that it cuts both pyramid (1) and dimetric prism (*i-i*), an eight-sided figure results (see fig. 1), in which the outlines of the pyramid will be referred to as those of the normal prism (*I*).

Such a section seen between crossed Nicols, whose principal sections lie parallel to the diagonals of *I* (position I), presents an appearance indicated by the diagram, fig. 1. There appears, namely, a dark square, whose sides lie parallel to the outlines of the prism (*I*), with dark lines running to the outlines of *i i*, and perpendicular to the same, thus coinciding with the diagonals of *I*. The dark square and lines are well defined. The outer zone, divided by the dark lines into four segments, is in position I at its maximum of brightness.

1.

On revolving the section through 45°, or until the principal sections of the Nicols coincide with diagonals of *i-i* (position II), the whole field becomes equally dark, and the interference cross of the calcite plate suffers no distortion in any part of the section.

The dark square and lines revolve with the section, and the darkening of the outer zone is gradual. It is in but few cases, however, that the action of a crystal agrees entirely with the above. The variations can, however, be most easily described and best understood by comparison with that action as a basis.

In the first place, the outline of the dark figure is seldom that of a true square, its sides being usually more or less broken lines, even when the angles remain nearly or quite 90°. It may, too, resemble a rectangle rather than a square. The dark lines subdividing the outer zone are less frequently variable than the outlines of the figure. In proportion as the form of the dark figure approaches the square in regularity, so is the optical behavior of the space enclosed uniform. In one case observed,

* J. Rumpf "Ueber den Krystallbau des Apophyllits" Min. und petr. Mitthelungen von G. Tschermak, Neue Folge, ii, 369 (1879).

a section placed in position II was wholly dark, excepting at one of the angles of the square, where a faint light was visible, producing distortion of the calcite interference cross at this point. In another section, in position I, the dark figure was not completely dark, there being light enough to admit of the distinct appearance of two perfect black crosses, whose thick bushy arms lay parallel to the diagonals of prism I, and which, revolving with the section, disappeared entirely in position II. All but transmitted light must be excluded, in order to see these crosses distinctly, as the whole of the square is still very dark in contrast with the outer zone. In cases where the boundaries of the dark figure are much-broken lines, the space within is commonly divided into a number of irregular patches, each with its black cross, seen in position I. In such cases, too, the whole field does not become uniformly dark in any position. The size of the dark figure relative to that of the section varies greatly. In most cases the relation is similar to that of fig. 1; while in some prismatic sections the dark square is larger than can be inscribed within the prism, its angles being cut off by the outlines of *i-i*. Again, the dark figure becomes very small, though in no case yet observed has it been entirely wanting.

It is impossible to indicate all the irregularities observed, within the limits of this article, and the fuller description of these interesting phenomena must be reserved for the final report on the region embracing Table Mountain.

None of the sections thus far prepared, parallel to the prism *i-i*, have exhibited any marked abnormal properties.

It is thought that the degree of variation, in the optical properties, from the simplest form illustrated by fig. 1, stands in intimate relation to the degree of irregularity in crystal growth indicated by the faceted surfaces. Certainly no hypothesis, however ingenious, which considers the tetragonal symmetry of apophyllite as a result of intricate polysynthetic twin structure of rhombic or monoclinic individuals (that of Rumpf l. c.), can explain the present case with a tithe of the plausibility with which the theory of *inner tension** is able to do it.

It is hoped that further investigations will prove the direct applicability of this latter theory to the present instance.

* As leading instances of the application of this theory to the explanation of optical anomalies in minerals may be mentioned:—

(*a.*) C. Klein, "Ueber den Boracit," Neues Jahrbuch für Mineralogie, etc., 1880, ii, p. 209.

(*b.*) C. Klein, "Zur Frage über das Krystallsystem des Boracits," ibid., 1881, i, p. 239.

(*c.*) Alfredo Ben-Saude, "Ueber den Analcim," ibid., 1882, i, p. 41.

(*d.*) F. Klocke, "Ueber Doppelbrechung regulären Krystalle," ibid., 1880, i, p. 53.

F. Klocke * in a critical review of Rumpf's hypothesis (l. c.), advocated the theory of inner tension (Spannung) in explanation of the anomalous optical behavior of apophyllite. He also showed that Rumpf's hypothesis could not correctly explain phenomena in apophyllite much simpler than those above described.

In chemical composition this apophyllite is quite normal, the fresh substance yielding the following:

SiO_2	51·886
Al_2O_3	1·540
Fe_2O_3	0·130
CaO	24·513
K_2O	3·809
Na_2O	0·590
H_2O	16·523
Fl	1·700
	100·691
O for Fl	0·716
	99·975

Considering all potassium and sodium as combined with fluorine, the following oxygen ratios are afforded:

RO	SiO_2	H_2O
1	3·95	2·09

The theoretical proportion 1:4:2 would be still nearer approached were it not for a probable slight loss of silica and excess of water, scarcely to be avoided in analyses of silicates containing fluorine. The Fe_2O_3 is undoubtedly owing to minute particles of limonite, which could not be completely removed. The Al_2O_3 is much higher than in most analyses, and the condition in which it is present seems undeterminable.

Much of the Table Mountain apophyllite has suffered alteration to a snowy-white substance resembling that commonly known as *albine*. Knop † has proved, for many cases at least, that this latter substance is calcite. At Table Mountain, however, although the end product resembles albine, it is in reality of totally different nature.

Up to the present time it has been impossible to procure the alteration product, sufficiently free from fresh or partially decomposed apophyllite, to allow of its exact determination. From analyses made, it is certain that Fl, the alkalies and Ca decrease markedly, while the percentages of SiO_2, Al_2O_3 and H_2O increase greatly.

* F. Klocke, review of Rumpf's article, "Ueber den Krystallbau des Apophyllits," in "Neues Jahrbuch," etc., 1880, ii, p. 11.

† In Blum, "Die Pseudomorphosen, etc., Dritter Nachtrag," 1863, p. 41.

There is no calcite in the product at all. The substance is light, has a pearly luster, and is finely foliated parallel to the basal plane of the apophyllite. The alteration proceeds from without. Further data concerning the substance and the process of alteration will be given in the final report.

5. Calcite.

The carbonate of calcium has had three periods of deposition in the basaltic cavities of Table Mountain—two as calcite, and one as aragonite.

In the form of wine-yellow crystals, it preceded even chabazite, being in all observed cases deposited directly on the basalt, and coated usually by chabazite or thomsonite.

It is rarely found in those cavities to which water has had access through fissures, having been dissolved.

The second deposit of calcite came after apophyllite. These crystals are colorless or slightly straw-yellow, and the form of both varieties is commonly that of a sharp scalenohedron terminated by a low rhombohedron.

The aragonite is present only as a snow-white incrustation, apparently with a special tendency to deposition upon chabazite, though often noticed on apophyllite and thomsonite. It was next to the last mineral deposited, only mesolite having been observed upon it.

6. Mesolite.

Mesolite is the last of the minerals deposited at the locality on North Table Mountain, where all of the species thus far described occur so often together, that their order of succession is plain (this Journal, June). The mineral appears uniformly in masses composed of exceedingly delicate needles, loosely grouped together, very much like the spicules of a fine sponge. Such light aggregates frequently fill the smaller cavities entirely. In the larger ones, the bases of the rounded bunches, 1 to 2 inches in diameter, often touch each other. The very latest deposition, the finishing touch so to speak, is a thin film, coating the whole mass. This is sometimes a continuous membrane, and in other cases more like a thick cobweb. The exquisite delicacy of some of these films is quite wonderful. In rare cases, bunches on the upper and lower walls of a cavity are united by such a membrane. Single needles are clear, but the aggregate appears pure white.

As was mentioned under thomsonite, the loose aggregates of the second generation of that mineral, seem specially suited to attract the deposition of mesolite.

None of the mesolite needles are large enough to allow any determination of their crystal form, even under the microscope at a high power. They seem simply like very fine transparent hairs. The identification as mesolite, rests therefore, on the following chemical analysis:

	a	*b*	*c*
SiO_2	46·138	46·020	46·333
Al_2O_3	26·880	26·870	
CaO	8·770		
Na_2O	6·190		
H_2O	12·168	12·169	12·130
	100·146		

Oxygen ratio:

RO	R_2O_3	SiO_2	H_2O
1	3·06	5·99	2·63

If mesolite be considered as a mixture of the isomorphous silicates contained in scolecite ($CaAl_2Si_3O_{10}+3$ aq) and natrolite ($Na_2Al_2Si_3O_{10}+2$ aq), the present occurrence would answer very nearly to the requirements of the combination of 2 of scolecite + 1 of natrolite, the percentages of which would be SiO_2 46·32, Al_2O_3 26·40, CaO 9·61, Na_2O 5·32, H_2O 12·35 = 100·00.

With mesolite ends the chief series of Table Mountain zeolites. The species described are for the most part clear or colorless, and well defined in crystallization, while they occur so often associated as to make the order in which they have been described a perfectly natural one.

A *second series* of zeolitic minerals will now be taken up, the members of which, in time and manner of deposition, and partially also in composition, are very distinct from those already mentioned.

In the previous article (this Journal, June) chabazite was stated to be "the oldest of the zeolites, with the exception of certain peculiar stratified deposits in some of the cavities." Since then the character of these deposits has been determined, and an interesting association of minerals discovered.

The attention of an observer in the zeolitic zone of Table Mountain is immediately attracted to a reddish-yellow sandstone-like material, which occurs in many of the cavities. In the larger ones it takes the form of a floor, the upper surface being horizontal, and the deposit may be several inches in thickness. Small cavities have been completely filled by it, and it is clear that the deposition has taken place from the bottom of each cavity, upward.

In parts of South Table Mountain especially, the same material has filled fissures. Usually the *lower* portion of such masses is composed of a reddish-yellow mineral, in irregular grains, which form a compact aggregate, in which lie isolated

spherules of a similarly colored radiate mineral. These spherules are seldom more than 2^{mm} in diameter, and are very perfect spheres. They increase in number upward and finally compose the greater part of the deposit.

In one cavity, 6 to 8 feet in horizontal diameter and about two feet in height, the deposit is quite different. Here the main mass is loosely granular, and is formed chiefly by a bright greenish-yellow mineral, while a stratified appearance is produced by layers of a white or colorless mineral. Some of the light layers are chiefly made up of easily recognizable stilbite, and the same mineral in distinct tablets forms the upper layer of the whole deposit. There are also irregular seams of white running through the yellow mineral.

The determination of the minerals in these deposits began with the greenish-yellow sand of this last mentioned cavity, and the description will follow the same order.

If the loose sand be placed under the microscope with a power of about fifteen diameters, it is seen to consist of prismatic grains, mostly with broken terminations. Many of the grains are clear and transparent, with the greenish-yellow color mentioned, while others are dull. The clear prisms polarize strongly, and extinction takes place at an angle of 35° to 40° with the vertical axis.

On splitting open some of the white layers, surfaces are obtained, showing minute, stout prismatic crystals, which, seen under the microscope, present the same habitus as the yellow grains. The prism angles are nearly 86° and 94°, and the termination is usually formed by an oblique plane, like a hemidome of the monoclinic system. The optical orientation is the same as in the yellow crystals, and corresponds also to the requirements of the monoclinic system so far as can be determined. The properties given agree with those of laumontite, and in the light of the following analyses, the identification of both yellow and white crystals with that mineral seems entirely justifiable.

	a Yellow grains.	*b* White crystals.
SiO_2	51·738	52·835
Al_2O_3	21·649	21·619
Fe_2O_3	0·947	
CaO	11·949	11·406
Na_2O	0·191	0·484
K_2O	0·352	0·424
H_2O	13·297	13·324
	100·123	100·092

Oxygen ratios:

	RO :	R_2O_3 :	SiO_2 :	H_2O
a	1 :	2·95 :	7·83 :	3·35
b	1 :	2·93 :	8·16 :	3·43

Aside from the H_2O, these ratios agree closely with the theoretical requirements of laumontite 1 : 3 : 8 : 4, especially as it can scarcely be supposed that the material was absolutely pure. It is quite probable that a small amount of stilbite was included with *b*, which would explain the high percentage of SiO_2.

In regard to the amount of water found it is only necessary to suppose that the dull particles have lost a part of their water, while the transparent grains are entirely fresh, in order to explain the relation of water to the other constituents as shown in the above ratios, a supposition in full accord with the characteristic tendency of laumontite to lose its water on exposure.

The mineral is not quite so easily fusible before the blowpipe as typical laumontite should be, according to the textbooks, but the difference is not sufficiently pronounced to be considered abnormal.

Turning now to the more compact reddish-yellow deposits, which correspond so closely to the one described, the same constituent minerals were sought for. Some small cracks or fissures, usually ending blindly in the yellow mass, were noticed, some of which were only partially filled with minute white crystals. On splitting the mass open, along such a half-filled crack, two surfaces were obtained, coated with minute, but exceedingly perfect clear crystals, easily recognizable under the microscope, as laumontite and stilbite. The little crystals of the former show occasionally the clinopinacoid, and a steep positive orthodome in addition to the prism and basis.

The sand obtained by simple fracture of the yellow, massive portion, on being placed under the microscope, is seen to consist largely of fragments of tabular crystals, the angles of which, so far as they are determinable, correspond to stilbite. The grains which are not evident fragments of tablets, are in part roughly prismatic, though seldom showing definite faces; neither could the optical action be satisfactorily determined.

Chemical analyses were made from two different specimens of the yellow granular mass, care being taken to exclude all reddish spherules. The results are given under *c* and *d* below.

	c	*d*	*e*
SiO_2	55·370	54·802	40·518
Al_2O_3	17·641	17 557	29·216
Fe_2O_3	0·790	0·754	0·788
CaO	8 525	8·412	12·427
K_2O	0·173	0·069	
Na_2O	1·429	1·506	4·306
H_2O	16·278	17·040	12·794
	100·206	100·140	100·049

The oxygen ratios are:

	RO	R_2O_3	SiO_2	H_2O
c	1	2·99	10·42	5·11
d	1	3·00	10·42	5·40

This composition is such as would result from a mixture of stilbite and laumontite, and although the latter mineral could not be positively identified in the sand examined with the microscope, there seems to be no good ground for doubting that it is actually present.

It seems remarkable that the material from two different cavities should contain the two minerals in so nearly the same proportions, as indicated by the analyses.

Concerning the reddish spherules, no data of importance could be obtained except by chemical analysis. Under *e* above is given the composition found for this substance in material which was apparently very pure.

The oxygen ratio for this, is:

RO	R_2O_3	SiO_2	H_2O
1	2·98	4·64	2·44

These figures agree so well with those obtained for the thomsonite of the first series (see Analysis I in June number, this Journal), viz:

1 : 3·09 : 4·76 : 2·51

that in absence of anything to the contrary, the identity of the two substances can scarcely be doubted. It was noticed here as in the other thomsonite, that about 2 per cent of the water could be expelled only at a very high temperature.

Thomsonite in the form of these reddish spherules has been deposited locally in great abundance in irregular cavities on the upper surface of the lower sheet of basalt, and also in the angular spaces, formed where the scoriaceous crust of the flow has been broken or crumpled. It is here deposited alone, free from stilbite and laumontite, but the spherules are exactly similar in size and appearance to those formed in the cavities below.

The ferric oxide in all these minerals seems to replace a portion of the alumina.

The occurrence of cavities containing these stratified deposits side by side with those entirely free from anything of the kind is very interesting. It seems to be explainable with plausibility on the theory that fissures formerly led into those cavities containing the reddish deposits, which were naturally followed by percolating waters.

The formation of zeolites in all cavities alike could only begin after the filling up of these fissures by the deposition of mineral matter. In support of this theory, it was noticed that

in no case were the reddish deposits formed upon any clear mineral of the first series described, and that in all cases where minerals of the first series were formed in cavities containing the stratified deposits, they occurred in the same order and manner as in any other cavity, being situated on the roof and sides as well as upon the floor. Fissures filled with these reddish minerals were in fact seen in several cases leading into cavities containing stratified deposits of the same minerals.

In a third article, still other zeolitic species from Table Mountain will be described.

ART. XVII.—*On a Property of the Isentropic Curve for a Perfect Gas as drawn upon the Thermodynamic Surface of Pressure, Volume and Temperature;** by FRANCIS E. NIPHER.

THE equation of this thermodynamic surface is

$$pv = \mathrm{RT}, \tag{1}$$

where p, v, T represent the pressure, volume and absolute temperature, and where R is directly proportional to the volume of a unit mass (or inversely proportional to the density) of the gas at a standard temperature and pressure.

By differentiation (1) becomes

$$dp = \frac{\mathrm{R}}{v} d\mathrm{T} - \frac{\mathrm{RT}}{v^2} dv \tag{2}$$

For convenience putting

$$\frac{\mathrm{R}}{v} = \mathrm{A}, \quad \frac{\mathrm{RT}}{v^2} = \mathrm{B},$$

and (2) becomes

$$dp = \mathrm{A}d\mathrm{T} - \mathrm{B}dv. \tag{3}$$

1°. To find the direction of maximum slope with respect to the v,T plane at any point on the surface. For this purpose pass a plane through any point in the surface, and at right angles to the v,T plane. Its trace upon the v,T plane is

$$\mathrm{T} = \beta + av, \tag{4}$$

p being indeterminate; where a is the tangent of the angle which the trace makes with the v axis, or

$$a = \frac{d\mathrm{T}}{dv}. \tag{5}$$

From (3) and (5) we have

$$dp = (\mathrm{A}a - \mathrm{B})\, dv. \tag{6}$$

* From Trans. of St. Louis Academy of Science, read April 3, 1882.

Calling S the slope of any element of the intersection of the plane and the surface, dz being the projection of the element on the v,T plane, we have

$$S=\frac{dp}{dz}=\frac{dp}{\sqrt{dv^2+dT^2}}, \tag{7}$$

which by (5) becomes

$$S=\frac{dp}{dv}\cdot\frac{1}{\sqrt{1+a^2}},$$

and by (6) we have further

$$S=\frac{Aa-B}{\sqrt{1+a^2}}. \tag{8}$$

In determining the direction of maximum slope at any point, it is evident that A and B will be constant, which gives as the required condition,

$$\frac{dS}{da}=\frac{A+Ba}{(1+a)^{\frac{3}{2}}}=0,$$

or

$$a=-\frac{A}{B}.$$

Substituting the values of A and B, we have

$$a=-\frac{v}{T}=-\frac{R}{p}=\tan i. \tag{9}$$

For very low pressures, the direction of maximum slope $\frac{dp}{dz}$ becomes more and more nearly at right angles to the plane of p,v; while for high pressures this direction becomes more and more nearly parallel to the plane of p, v. The direction of maximum slope is constant along a line of constant pressure.

2°. To find the direction of the isentropic line at any point on the surface, as related to the direction of maximum slope determined in (9).

Poisson's equation :

$$Tv^{k-1}=\text{const.} \tag{10}$$

is a projection of the isentropic line upon the plane of v, T, where k is the ratio of the specific heats = 1·41.

Calling a' the tangent of the angle which any element of this projection makes with the v axis, we have

$$a'=\frac{dT}{dv}=\tan i'.$$

This value of a' is obtained by differentiating (10) and is found to be

$$\frac{dT}{dv}=-\frac{T}{v}(k-1)=-\frac{P}{R}(k-1). \tag{11}$$

Here also the condition of constant pressure gives a constant value for a'. Hence, at any point along any line of constant pressure the projection of an element of the isentropic line, upon the v, T plane, makes a constant angle with the projected line of greatest slope at the same point.

From equations (9) and (11) it follows that

$$\tan i' = \frac{k-1}{\tan i}; \tag{12}$$

from which it will appear that for either very high or very low pressure the isentropic line runs at right angles to the direction of greatest slope. The condition that it shall coincide with the direction of greatest slope is

$$\tan i' = \sqrt{k-1} = \frac{\mathrm{R}}{p},$$

or

$$p' = \frac{\mathrm{R}}{\sqrt{k-1}}. \tag{13}$$

For air this pressure is about 3·2 millimeters of mercury, and for other gases it is proportional to the volumes of a unit mass, at a standard temperature and pressure.

The thermodynamic surfaces of various gases will lie the one above the other, those having the largest value of R being uppermost. If we now substitute the value of p' of (13) in the original equation of the surface, we have

$$v = \sqrt{k-1}\,\mathrm{T}, \tag{14}$$

which is independent of R. Hence, for all gases which follow the law represented in (1) the lines on their respective surfaces, where the isentropic lines coincide with the direction of maximum slope (13), will all lie in a common plane passing through the axis of P and at right angles to the plane of v, T, its trace upon the latter plane being represented by (14).

If the gases have a common temperature while in this condition, (14) shows that they will also have a common density, which when T is 273° will be 0·000058 grams to the cubic centimeter.

It will be observed that for air, the pressure indicated in (13) is practically the same as that at which Maxwell's law for viscosity begins to fail. This, however, is a mere coincidence. The two phenomena have nothing in common, as is evident both from theoretical considerations and from experimental results.

Washington University, March 30, 1882.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Constitution of Solutions.*—Krüss has applied the spectroscope to the determination of the constitution of solutions. It is well known that the absorption-spectrum of a solution containing two or more colored substances does not correspond in all cases to the sum of the separate spectra. Whether this has a chemical or physical cause has not been settled, though the former is generally assumed. For the comparison of single spectra, Krüss used absorption cells with parallel glass sides, the width of the cell being exactly 5^{mm} and the sides being 2^{mm} thick, and called a. Two of these cells, containing different liquids and placed one behind the other, gave the physically mixed spectra. For the chemically mixed spectra the two solutions were mixed in the proportion of 1:1 and placed in a glass cell whose interior width was $10+a^{mm}$, and whose glass sides were a^{mm} thick. A glass plate of thickness a was placed within the liquid and a second one outside, both being perpendicular to the light-rays. The liquid layer has thus the same thickness as in the previous case, 10^{mm}, the glass absorbing layer has the same thickness, $4a$, and there are as before 4 reflections between liquid and glass and 4 between air and glass. As an illustration, the action of aqueous solutions of pararosaniline (fuchsine), and trinitrophenol (picric acid), is described. The former showed the strong absorption bands from $\lambda=570{\cdot}4$ to $518{\cdot}2$ the intensity diminishing—with the exception of a band $\lambda=485$—toward the more refrangible end to $\lambda=429{\cdot}6$, where it again suddenly increased. The latter showed no distinct color between $\lambda=483{\cdot}8$ and the violet. When the two cells were interposed, the strong bands at $\lambda=570{\cdot}4-518{\cdot}2$ appeared, and also the strong absorption beginning at $\lambda=483{\cdot}8$. When a mixture of the two solutions was examined, the absorption bands $\lambda=570{\cdot}4-518{\cdot}2$ had disappeared and a pretty strong absorption from $\lambda=576{\cdot}9$ toward the violet had taken its place, while from $\lambda=483{\cdot}8$ no distinct color could be recognized. These changes have unquestionably a chemical origin. If, however, solutions of neutral potassium chromate and ammonium-cupric sulphate be mixed, no distinction can be observed qualitatively between the spectrum of the mixture and the sum of the two single spectra. When these spectra are examined quantitatively by Vierordt's method, on the other hand, using a solution of the former salt of the concentration $y=0{\cdot}01805$ grams in 1^{cc}, and one of the latter of $x=0{\cdot}02$ grams, the superposed spectra show in the region E26F—E45F, a light-intensity of $0{\cdot}2366$ and E63F—E80F of $0{\cdot}361$, corresponding to the extinction-coëfficients $\varepsilon_1=0{\cdot}52784$ and $\varepsilon_2=0{\cdot}44250$. These coëfficients, calculated by Vierordt's formula from his data, are identical with the above values within the limits of error. In

the region E26F−E45F, the absorption-constant for ammonium-cupric sulphate is α=0·02192 and for potassium chromate β=0·1292; and in the region E63F−E80F it is γ=0·03472 for the former, and δ=0·05937 for the latter. From the formula $\varepsilon_1=\frac{x}{2\alpha}+\frac{y}{2\beta}$, the value of ε_1=0·52611 for the former region, and $\varepsilon_2=\frac{x}{2\gamma}+\frac{y}{2\delta}$=0·44003 for the latter. But when the two solutions are mixed, the extinction-coëfficient ε_1=0·21468 and ε_2= 0·07573. These values differ so much from the others as to justify the conclusion that a chemical action of some kind has taken place. If solutions of potassium chromate and dichromate, or of permanganate and dichromate are mixed, the spectrum of the mixture is identical with that of the sum of the single spectra. Hence the author concludes that spectrum analysis gives a convenient, rapid and certain method of determining whether in solutions containing two or more coloring matters these substances act chemically on one another or may be mixed together without any chemical change taking place.—*Ber. Berl. Chem. Ges.*, xv, 1243, June, 1882. G. F. B.

2. *On the Vapor-density of Bromine.*—Jahn has determined in Ludwig's laboratory the vapor-density of bromine. The material was carefully purified, and had a constant boiling point of 63·07° C. The vapor-density was determined by Bunsen's method somewhat modified. The first series of determinations was made at 102·6° C., and gave (*a*) 5·7225, (*b*) 5·7388, (*c*) 5·7228; mean, 5·728. The second series, at 131·92° C., gave (*a*) 5·635, (*b*) 5·646, (*c*) 5·638; mean, 5·640. The third gave at 175·58° C., (*a*) 5·603, (*b*) 5·605; mean, 5·604. The fourth, at 210·32° C., gave (*a*) 5·543, (*b*) 5·549; mean, 5·546. The fifth, at 227·92° C., (*a*) 5·5241, (*b*) 5·5245, (*c*) 5·5244; mean, 5·5243. Taking Stas's value for the atomic weight 79·951, the calculated vapor-density is $\frac{159\cdot902}{28\cdot943}$=5·5247; a value practically identical with that last given. If further the expansion be assumed to increase with the temperature and in the linear formula $D=a+bt$, the value of the constants a and b be calculated from the above data by the method of least squares, a is found to be 5·8691 and b= −0·00153. Substituting in the formula, the calculated values are obtained for the above temperatures. They agree with the experimental values very well except at 102·6° and 131·92°. Here the expansion curve is a conic section, the temperature being not far above the boiling point. Using, therefore, the quadratic formula $D=5\cdot5189-\frac{8\cdot535}{t}+\frac{3190\cdot04}{t^2}$ the calculated values come out 5·729 at 102·6° and 5·638 at 131·92°. Beyond this point the linear equation holds. Calculating now the vapor-density of bromine at temperatures exceeding its boiling point by intervals of 20°, the author finds that the deviation from the nor-

mal value becomes practically zero at 160° above this boiling point, chlorine reaching its normal density at 240° above its boiling point, and iodine at its boiling point. Jahn attributes this to molecular aggregation, the molecules becoming single and separate only at temperatures at which the vapor-density becomes normal.—*Ber. Berl. Chem. Ges.*, XV, 1238, June, 1882. G. F. B.

3. *New method for preparing Hyponitrous acid.*—For the production of hyponitrous acid, HNO, the agents hitherto used for the reduction of the nitrate or nitrite have been sodium-amalgam or electrolysis, mercury being the negative electrode. ZORN has made a series of experiments on the subject and comes to the conclusion that the best reducing agent for this purpose is ferrous hydrate. When freshly precipitated ferrous hydrate is placed in a solution of sodium nitrate or nitrite, it reduces it energetically, evolving heat and becoming ferric hydrate. There results hyponitrous oxide, hyponitrous acid, ammonia and nitrogen. In practice, pure ferrous sulphate is dissolved in water and mixed with milk of lime, avoiding an excess, and leaving the solution slightly acid. To this thin magma is added, a solution of sodium nitrite (one part of the nitrite being used for every 10 parts of the ferrous sulphate) and the whole is well cooled. The mass foams, and the reduction is effected in a few hours. It is strained and filtered, carefully neutralized with acetic acid, and precipitated with silver nitrate. The precipitate of silver hyponitrite is perfectly pure since by this method of reduction no oydroxylamine is formed, and therefore no metallic silver is mixed with the precipitated hyponitrite. From 100 grams nitrite and one kilometer of ferrous sulphate, 10 grams pure silver hyponitrite was obtained.—*Ber. Berl. Chem. Ges.*, XV, 1258, June, 1882. G. F. B.

4. *On an Apparatus for Liquefying Gases.*—REYNOLDS has described a simple form of apparatus for liquefying ammonia gas, which may be used with other gases as well. It consists of a U-tube, made of gas pipe, the sides of which are 12^{cm} internal diameter, and the bottom pipe 5 or 6^{mm}. One of the sides is 40^{cm} long, the other is 30^{cm}; the bottom piece is 25^{cm} in length, and is screwed firmly at its ends into caps on the lower ends of the side tubes. Upon the upper end of the shorter side a cap is screwed, fitted to support a glass Cailletet tube, the capillary portion of which rises above its top. The upper end of the longer side is also furnished with a cap. The Cailletet tube having been filled with pure dry ammonia gas in the usual way, is immersed in the mercury, with which the apparatus has been filled, and the cap screwed on. Mercury is then removed from the longer leg, leaving a space of 12^{cm} between the metal and the cap. This space is then filled with the strongest solution of ammonia, and the cap is firmly screwed on. If now this tube be heated gradually with a Bunsen burner, the ammonia gas set free by the heat exerts pressure upon the mercury and so compresses the gas in the Cailletet tube until finally liquefaction is produced and a layer of

the liquefied gas appears in the capillary portion of the tube. On cooling, the gas is reabsorbed.—*J. Chem. Soc.*, xli, 259, June, 1882. G. F. B.

5. *Function of two ears in the perception of space.*—Professor Silvanus P. Thompson concludes from a review of rival theories that "Judgments as to the direction of sounds are based, in general, upon the sensations of different intensity in the two ears; but the perceived difference of intensity upon which a judgment is based is not usually the difference in intensity of the lowest or fundamental tone of the compound sound ("or clang") but upon the difference in intensity of the individual tone or tones of the clang for which the intensity-difference has the greatest effective result on the quality of the sound."—*Phil. Mag.*, June, 1882, pp. 406–416. J. T.

6. *Vapor tension of mercury.*—At a meeting of the Physical Society in Berlin, May 26, Dr. Hagen discussed the results of Regnault on the vapor tension of mercury, and gave the results of his own determinations. Dr. Hagen's apparatus consisted of a V-shaped tube, having at the lower part a long straight tube united to it by fusion, while above either branch terminated in a tube twice bent at a right angle, and closed at the lower end. By means of a Hagen air-pump this tube system was gradually evacuated to a pressure of 1–12,000,000 mm. mercury, and the long straight tube opened under mercury at the lower end. The mercury rose in both branches of the V-tube to barometric height. One of the lateral ends of the apparatus was now kept constant at 0°; while the other was first cooled to −42°, and then heated to various temperatures; each time the position of the mercury in the two branches was observed with a cathetometer, and the difference of their heights gave the vapor tension. The values so obtained were less for all temperatures than those given by Regnault. Dr. Hagen concludes "that the Regnault values for the vapor tension of mercury, which have passed into all text books, are too large."—*Nature*, June 15, 1882. J. T.

7. *Electrostatic dimensions of a magnetic pole.*—Clausius gives his reasons for believing that Maxwell's expression for the dimensions of the unit of static magnetism, viz: $[m_s]=[M^{\frac{5}{2}}L^{\frac{1}{2}}]$ is wrong and that the true value of this unit should be $[m_s]=[M^{\frac{1}{2}}L^{\frac{5}{2}}T^{-2}]$; Professor J. J. Thomson defends Maxwell and shows that Maxwell's value results from the introduction of μ the magnetic permeability of the medium. Mr. W. D. Niven suggests that the value given by Clausius for the dimensions of a magnetic pole does not make the magnetic force between two such poles of the dimensions of a force, which ought to be the case.—*Phil. Mag.*, June, 1882, pp. 381–398 et 427–429. J. T.

8. *The electricity of flames.*—Several observers have investigated the electricity of flames and have arrived at various conclusions, which may be classed as follows:—

(1.) The electricity of flames depends upon the process of combustion (Pouillet, Hankel).

(2.) The flame behaves like an electrolyte to the metal electrodes which are immersed in it (Matteucci).

(3.) The explanation of the electricity of flames is found in the thermoelectric difference of the electrodes (Buff).

Julius Elster and Hans Geitel believe that previous observers have overlooked the influence which the heated layer of air outside the flame exerts. On account of the great resistance of this air, observers who have used a galvanometer could not detect a difference of potential. They, however, used a Thomson's electrometer and were enabled to study the phenomena, believing that they were the first to use a Thomson's electrometer for this purpose. The same method, however, was used by J. Trowbridge (this Journal, 1873).

Their conclusions are as follows:

(1.) The length polarization of the flame (Hankel) is only an apparent phenomenon and is due to the unequal immersion of the wires which serve as electrodes.

(2.) The flame appears in section to be strongly polarized—and the electrode in the surrounding layer of air appears to be always positive toward the electrode in the flame.

(3.) The electromotive force is independent of the size of the flame.

(4.) The change of polarity of the flame is due to change of position of the electrodes.

(5.) The electromotive force of the flame is dependent upon the nature of the metal from which the exploring electrodes is made, and also upon the nature of the burning gas. Aluminum and magnesium call forth strong electrical effects. If the electrode in the layer of air is covered with a salt the electrical effect is very weak.

(6.) With the use of water electrodes and exclusion of metals, the electrical phenomenon of flames is also seen. The electrode in the outer layer of heated air is always positive to that in the flame itself.

(7.) Flames can be combined in series just as batteries are joined for intensity.—*Ann. der Physik und Chemie*, No. 6, pp. 193–222, 1882. J. T.

9. *Kerr's Phenomenon.*—H. Brongersma has repeated Kerr's experiments on "A new relation between electricity and light," Phil. Mag., No. 4 (50), p. 337, 1875, and has corroborated Kerr's results which certain observers have failed to repeat. Brongersma agrees with Röntgen that Kerr's phenomena has a fundamental importance in connection with the electrodynamic theory of light.—*Ann. der Physik und Chemie*, No. 6, pp. 222–233, 1882. J. T.

II. Geology and Mineralogy.

1. *On the structure and movement of Glaciers.*—M. F.-A. Forel, of Morges, Switzerland, has recently published (Bibl. Univ., III, vii, 329), an important memoir upon glaciers, embodying the results of observations by himself and M. Ed. Hagenbach-Bischoff, with a discussion of these results, and also of those obtained by other observers. His argument rests mainly upon the well attested fact that glacial ice has a distinctly crystalline granular structure, the mass being composed of a confused agglomeration of individual crystals, each optically distinct; and moreover, that the size of these crystalline grains increases from the upper margin of the glacier, at the limit of the névé, where they have the size of a hazel nut, down to the middle part, where the size is that of a walnut, and farther down to the extremity, where they are as large as a hen's egg. For example, at the lower extremity of the Aletsch glacier, or that of the Rhone, the grains have a diameter of 7 or 8 centimeters. In regard to this gradual increase in size of the crystal-individuals, the author remarks that two suppositions are possible: either the growth of some grains must go on at the expense of others less favorably situated, one gaining what the next loses, and absorbing as much heat as is disengaged by the crystallization; or, each grain increases in size by means of the water which reaches it from above from the surface of the glacier. Of these two hypotheses, the first is rejected on the ground that wherever observations have been made, they have shown the grains to be all of sensibly the same size in the same region, and not to be some small, others large, as this explanation would require.

Accepting provisionally the second hypothesis, the author remarks that for the increase in volume of the crystals there are needed: water, cold and favorable conditions. About the last point nothing is definitely known, but the others admit of further discussion. The water is believed to be afforded by the melting of the upper surface of the glacier under the influence of the heat of summer. This water runs over the surface of the ice, descends into the crevasses, and, if it be admitted that the ice contains capillary fissures, a point which is discussed later, much of it would be absorbed by the mass of the glacier and used in increasing the size of the crystalline grains; the rest of the water flows off in the sub-glacial torrent. The low temperature needed for the solidification of the absorbed water is believed to be due to the continued loss of heat during the winter, the glacier as a whole being a mass, the temperature of which can never be above zero, but may fall considerably below. The question as to the mean temperature of the ice at different seasons of the year is discussed at length, and the author concludes, for a variety of reasons which cannot be quoted here, that the middle of the mass of the glacier has probably a temperature at the end of the winter several

degrees below 0° C. This excess of cold would be partially expended in causing the solidification of the water which, as already stated, is absorbed into its mass and thus goes to increase its volume. The crystalline grains are then to be conceived as growing by accretion, successive layers being added to them, at the expense of the water derived from surface melting, and in the process of the warming of the glacier which goes on during the summer.

Assuming the correctness of the results of Hugi, as to the increase in size of the crystalline grains, that is, in brief, that they increase from a diameter of 1 to one of 4 centimeters, taking 100 years for the time of their development, the author finds that the annual increase in volume is 4½ p. c. Assuming further that the cold of winter is all employed in bringing about this increase, it is calculated that the hypothesis advanced is satisfied if the temperature of the glacier descends in winter to −6°·8 C., or in round numbers −7° C. This temperature, the correctness of which is obviously dependent upon the accuracy of the assumed data as to the rate of increase of volume, is too low to be accepted and leads to the inference that a part of the increase is accomplished by a process different from that which has been described. Thus, at the end of the summer a considerable portion of the glacier must be at the temperature of melting ice, and in the capillary fissures between the crystalline grains there must be water; now as the glacier cools down in the autumn, the first effect of the loss of heat would be the solidification of this water and the consequent increase in size of the crystalline grains. Taking into account this last point, the author regards that the temperature that would have to be assumed for the glacier at the end of the winter would be quite within the range of possibility.

The hypothesis, which has been advanced, depends upon the assumption that the water can find its way into the interior of the glacial mass through the capillary fissures separating the individual grains. This point is one which is yet somewhat doubtful, and the author after considering the various observations of Agassiz and others, which tell for and against the possibility of such a penetration of the water, discusses the question from a more theoretical standpoint, and concludes that the assumption of the impermeability of the glacier is contrary to fact. He promises further to make this a special subject of observation at a later period.

In regard to the cause of the movement of glaciers, M. Forel places himself on the side of Hugi and Grad in supporting the theory of expansion, although modifying somewhat their hypothesis. On the old dilatation theory, it was the expansion of the water contained in the capillary fissures at the moment of their solidification, to which the glacial movement was supposed to be due. According to the view of M. Forel, however, this special expansion plays a subordinate part, and it is rather the gradual increase in volume of the crystalline grain due to the molecular

affinity which causes a crystal to grow in the mother-liquor in which it is placed.

In discussing further the application of the hypothesis, a distinction is made as to the course of events during the youth and during the old age of the glacier. The glacier may be divided into three parts. The first is in the elevated region where the glacier has its commencement, that of the névé. Here the heat of summer is not sufficient to melt the whole volume of the snow which falls during the year; only a part of the snow is consequently transformed into water and this penetrates into the layers below and is solidified there; the temperature is much below the freezing point. This is the region of the infancy of the glacier. Following this comes the line of separation where the heat of summer is just sufficient to melt the winter's snow, and there is no excess of heat to attack the ice.

The second stage, that of the youth of the glacier, is found below this line of separation where the summer's heat not only melts the snow, but also partially melts the ice; the water so formed is absorbed and assimilated by the ice, and the temperature below the surface is even at the end of summer below zero. In this region the glacier is increasing in volume and consequently moving downward. Then follows a second line of separation, where the water absorbed is all used in the increase of volume of the glacial grain. At this point the sub-glacial torrent has its origin, and at the summer's end the temperature is at 0°.

The third stage is that of the old age of the glacier, where the supply of water exceeds that needed to bring the temperature of the ice back to 0°, the excess of water flows off in the glacial streams; the temperature of the ice is at 0° during the summer and the excess of the summer's heat goes to cause the melting and destruction of the glacier.

In concluding his interesting memoir, the author promises to test his hypothesis by further observations and experiments, bearing especially upon the questions as to the comparative size of the crystalline grains in the different parts of a glacier, and as to the possibility of the penetration of the surface water into the mass of the ice.

2. *Upper Silurian fossils in the metamorphic rocks of Bergen, Norway.*—The discoveries of fossils in metamorphic rocks are increasing in numbers with the extension of careful observations. Mr. Hans H. Reusch has a memoir on new discoveries of this kind in the peninsula of Bergen, illustrated by a colored geological map and plates, which is published by the University of Christiania, under the direction of Prof. Kjerulf. Bergen is in 60° N., on the west coast of Norway. The predominant rock is granitoid gneiss. With the gneiss, in conformable strata, occur various schists, with nearly vertical dip—dioryte schist with included beds of granulyte and gneiss, labradorite rock, argillitic and ordinary mica schist, chloritic mica schist, hornblende schist more or less chloritic, and in some parts epidotic and calciferous

gneiss; also thin strata of a conglomerate made up of compressed pebbles or stones, a feldspathic quartzyte, and intercalated layers of crystalline limestone containing fossils. In addition the region includes in parallel position with the rest and adjoining the calciferous gneiss, a belt of "saussurite gabbro or greenstone." The strike of the whole is about E.N.E.; and, according to the author, the limestone or marble, the various kinds of schist, including the gneiss and granulyte, together with the quartzyte and conglomerate, appear to make one continued metamorphic series.

He says: The various gneisses which appear to be included within the Silurian terrane, I am inclined to regard as sedimentary, strata originally formed of loose material such as gravel and sand of granite or gneiss origin; and the argillitic and ordinary mica schists, sometimes porphyritic, in which occur Trilobites and other animal remains at Vagtdalen, as once beds of clay or mud; the conglomerates as owing their thin and sometimes lance-shaped pebbles to the pressure attending the metamorphism; and a green gneiss of Trengereid as probably an altered compressed breccia.

The argillitic mica schist [hydromica schist?] occurs in southern Bergen in two zones; in both of them the fine grained argillitic schist graduates into coarse mica schist (muscovitic), and includes layers of gneiss; and *both are fossiliferous.* The fossils found near Osören at Kuven, in lenticular masses of limestone, are *Halysites catenulata*, a species of *Cyathophyllum*, and a tubular fossil probably *Syringophyllum organum;* and at Valle, sections of Gasteropods referred to *Murchisonia* or *Subulites.* In the northern of the two zones the argillitic mica schist is associated with quartzyte or quartzytic sandstone, a conglomerate partly chloritic, "saussurite gabbro," and hornblende schist. The argillitic mica schist contains scattered individuals or crystals of brownish mica transverse to the foliation; it has afforded *Halysites catenulata*, a *Favosites*, a *Cyathophyllum*, trilobites of the genera *Calymene* and *Dalmanites*, and *Brachiopods*, apparently indicating the age to be "the lower part of the Upper Silurian;" and the schist or slate of Ulven water has afforded Graptolites of the genera *Rastrites* and *Monograptus.* Figures are given on plates 1 and 2, showing that the fossils are not doubtful appearances.

In the northeastern part of Bergen, at Trengereid, there are, along with layers of fine-grained gneiss, calciferous mica schist, granulyte, quartzyte, and a conglomerate made of compressed pebbles; there are also five beds of reddish marble in which are found hollow cylinders that appear to be *crinoidal stems*, and a "closely netted *chain-coral*," showing that the beds are of the same horizon as those of Osören.

3. *Geological Examinations in Southern Colorado and Northern New Mexico*, of 1878 and 1879, by J. J. Stevenson, Prof. Geol. Univ. City of New York, with an Appendix *upon the Carboniferous Invertebrate Fossils of New Mexico*, by C. A. White, M.D. 420 pp. and App. of xxxvi. 8vo, with four plates and many wood cuts. Report of the U. S. Geological Survey West of the 100th

Meridian, in charge of Capt. G. M. Wheeler, Engineer Corps, U. S. Army. Washington, 1881. (Distributed in June, 1882.)—Professor STEVENSON's Report treats of the geology of North Central New Mexico and South Central Colorado, between 37° 20′ N. and 35° 20′ N. and west of 104° 7½′ W., a region along the "Spanish Ranges" and a continuation of the Sangre de Cristo Range—having the Rio Grande on the west and headwaters of the Arkansas on the east. To the north the mountains are high and sharp, one peak over 14000 feet, while the southern part is comparatively gentle in slopes and crossed by roads. He describes the central portion of the mountain region from north to south as consisting mostly of Archæan rocks; on the east side and part of the top, Carboniferous beds, resting on the Archæan; next east, overlying the Carboniferous or else the Archæan, Cretaceous strata of the Dakota, Colorado and Laramie groups, excepting some doubtful sandstones that may be Jura-Trias; and of Tertiary beds in the vicinity of Galisteo Creek, New Mexico. Besides these rocks there are large areas of basaltic lavas with dikes and craters of Miocene age or later; and others of trachytic rocks which are not as old as the Laramie beds, since in some places they overlie these.

Prof. Stevenson devotes many pages to the Laramie or Lignitic group, and follows his descriptions with a discussion of its relations to the Cretaceous and Tertiary. New facts from the author's observations bearing on this disputed point, are brought forward, and the conclusion strongly urged that the formation is the true Upper Cretaceous. The close stratigraphical relations to the Upper Cretaceous and the similarity in the presence of lignitic beds are facts now generally admitted. But Professor Stevenson observes, beyond this, that while the Laramie is largely brackish-water and fresh-water in origin, as usually stated, typical fossils of the Cretaceous of the Fox Hills group (the upper Cretaceous of Hayden), are occasionally obtained, as at Evans and Greeley, as well as along Saint Vrains and Thompson Creeks in Colorado, from the very summit of the Laramie. The query arising here is whether the beds with the Fox Hills fossils are not distinctively of the Fox Hills group. Professor Stevenson's Report after treating with much detail of the topography, displacements and stratigraphy of the region, including a section on the glacial phenomena of the region, closes with a chapter on its Economic geology. An early and important chapter reviews the labors of other investigators over the region of his own work.

Dr. C. A. White's Appendix on the Carboniferous Invertebrate fossils, contains notes on a large number of species collected by the expedition in 1877 and 1878, and by Professor Cope in 1874. They are from New Mexico, and represent, according to Dr. White, rather the Upper part of the Coal Measures than the Middle or Lower. He states that nearly 100 species are identical with species found in the Coal Measures east of the Mississippi. A species of Rotella here described, *R. verruculifera* White, is the first of this genus yet recognized from the Carboniferous. The

previously described species of the genus are mostly, if not wholly, Tertiary and recent.

4. *Manual of the Geology of India:* Part III, *Economic Geology*, by V. Ball, M.A., F.R.S., Officiating Deputy Superintendent Geological Survey of India. 664 pp. Royal 8vo, with maps and plates. Calcutta, 1881. (Office of the Geol. Survey of India: London, Trübner & Co.)—The Parts I and II of this very valuable manual, by H. B. Medlicott and W. T. Blanford, were noticed in volume xix of this Journal (1880). The last Part, by Professor Ball, is devoted to the economical branch of the subject. This large volume is full of important matter on the metals and metallic ores, precious stones, coals, graphite, petroleum, amber, salt, and other useful materials, afforded by the rock formations of India. The history of the mining operations and discoveries in connection with each product from early time has been worked up, and a large amount of curious and valuable half-lost information brought together, the gathering of which from scattered records cost the author great research. The native processes are described in detail and illustrated by sketches of the workmen at their work, some of which are entertaining as well as instructive. Among the maps one is a large and beautiful topographical map of India; another, similar, illustrates the distribution of the coal mines; and others represent the diamond-bearing districts.

The volume is history, science and popular description combined. As the Superintendent of the Geological Survey of India, Mr. Medlicott, says in his preface, "The student as well as the man of enterprise will long owe the author gratitude for the great store of facts thus brought within easy reference."

5. *Report of Progress of the Geological Survey of Canada* for 1879–80, Alfred R. C. Selwyn, Director, Montreal, 1881.—The Canada Geological Reports always contain facts of wide geological bearing, and for the special reason that the region under investigation is of great extent and diversity, although the corps of observers is small. This new volume, after a review of the general results by Mr. Selwyn, the Director, has a chapter by the same on boring operations in the Souris River Valley, near the Rivière des Lacs, 229 miles west of Red River. Next follows a Report on the Lignite Tertiary formation from the Souris River to the 108th meridian, by G. M. Dawson, in continuation of that by the same author in his Report on the Geology of the 49th parallel connected with the Reports of the Boundary Commission. Thirteen analyses of the lignites give for the average, 41·10 fixed carbon, 41·41 volatile combustible matter, 5·55 ash, with 12 per cent of water. The clay iron-stones accompanying in thin layers the coal beds, contain the iron mainly as carbonate. The next paper, by Dr. Dawson, on the plants of the Lignite Tertiary of Roche Percée, Souris River, describes leaves 1½ feet long and broad of *Platanus nobilis* Newberry, a new Sassafras, *S. Selwyni*, *Taxites Occidentalis* Newberry, *T.*

Olriki, of Heer, who has described it from Alaska, Greenland and Spitzbergen. After these there are: a Report by G. M. Dawson on an Exploration from Port Simpson, on the Pacific coast, to Edmonton, on the Saskatchewan; on Hudson's Bay, and some of the lakes and rivers lying to the west of it, by R. Bell, containing also a paper on the northern limits of trees in Canada, east of the Rocky Mountains, a list of fossils collected by Dr. Bell in Manitoba, by J. F. Whiteaves, and other papers; also Report on New Brunswick, by R. W. Ells; on Nova Scotia, by H. Fletcher; on the Magdalen Ids., by J. Richardson, and chemical contribution to the survey, by G. C. Hofmann. The volume is illustrated by many maps, plates and sections.

6. *The Færöe Islands.*—Professor James Geikie has a paper on the geology of the Færöe Islands in the Transactions of the Royal Society of Edinburgh for 1882. The principal rocks are bedded basalts with intercalations of tufas, and in Myggenæs and Suderöe of shale and coal. The basalt, with the shale and coal, are referred to the Miocene. The basalt of Suderöe is mainly a chrysolitic dolerite, mostly fine-grained and rarely porphyritic; a bed sometimes has above a scoriaceous crust, or passes into a jumbled mass of fragments of scoria. The same rock on the northern islands, unless amygdaloidal, is more coarse grained and porphyritic; but amygdaloidal and non-amygdaloidal areas frequently alternate in more or less regular beds parallel to the bedding. The thickness of the basalts is stated to be 9,000 to 10,000 feet above the coal and 4,000 feet below the coal. The islands are glaciated, showing scratches and *roches moutonnées*, and indicating a movement for the most part to the southwest, but partly to the southeast. In the northern islands the thickness of the ice was 2,200 or 2,300 feet, and at least 1,400 on Suderöe; and judging from this thickness, the glacier probably reached out to the 100-fathom line.

7. *Ammonites in the Tejon Group of California.*—In a note on the Tejon Group of California, by A. Heilprin (Proc. Acad. N. Sci. Philad., 1882, 94), the Tertiary features of the fossils of the Tejon Group,—made the upper portion of the Cretaceous by Whitney, and also by Gabb—is remarked upon, and the occurrence in it of an Ammonite (*A. jugalis* Gabb) is mentioned as a case of the genus Ammonites extending into the Tertiary.

This Ammonite was found by Gabb also in the underlying beds of undoubted Cretaceous age, called by Whitney the Martinez group (Rep. Pal. Calif., vol. ii, p. 134). The writer, in his Geology (p. 458), recognizes the Tertiary features of the fossils described by Gabb, and to show it gives a complete list of the species (p. 508); but at the same time suggests that the beds are the equivalent of the Laramie or Lignitic beds, which also are strikingly Tertiary in the mollusks, though most geologists now make them the top of the Cretaceous. The Tejon group is the only lignitic portion of the California Cretaceous. J. D. D.

8. *Paleontology of the Brazilian Geological Survey.*—A letter to the editors from Dr. C. A. White states that he has finished two "Contributions for the Brazilian Survey, on the Cretaceous Conchifers and the Cephalopods, leaving the Gasteropods yet to be studied." He has found a new species of the genus Meekia of Gabb, which he has named *Meekia commemorata*, in commemoration of the able and greatly esteemed American paleontologist, Mr. Meek.

9. *Geological Sketches at Home and Abroad*, by ARCHIBALD GEIKIE, Director General of the Geological Surveys of the United Kingdom. 332 pp., with illustrations. 1882. London and New York. (Macmillan & Co.)—Geological descriptions, illustrating well the keen eye of the author as well as the "restless energy of nature," landscape sketches almost as living as nature herself, fragments of entertaining history, and amusing incidents of travel, are combined in these collected essays of the accomplished author in a manner fitted to attract all readers. The "Old Man of Hoy" is an account of British sea-coast scenery, and the various work of the waves among different kinds of rocks; "The Baron's Stone of Killochan" discourses about bowlders, glaciers, and the delightful scenes of a region within sight of the Firth of Clyde. And similarly, the chapters, "Among the Volcanoes of Central France," "the Old Glaciers of Norway and Scottand," "A Fragment of Primeval Europe," "The Scottish School of Geology," "Geographical Evolution," and others in the work put instruction in an attractive form. Some of the best chapters are those entitled, "In Wyoming," and "the Geysers of the Yellowstone"—regions visited by Professor Geikie in the summer of 1880. Another also, "the Lava-fields of Northwestern Europe," brings in American facts, and in a way to illustrate, most effectively, phenomena of Britain and Europe, as already shown to the readers of this Journal by an extract in volume xxi (1880, p. 145). Science, throughout the work, is made the genial, amusing and knowing companion of the reader.

10. *Columbite, Orthite and Monazite from Amelia Co., Virginia.*—The description by Prof. DUNNINGTON of microlite from Amelia Co., Va., has been noticed in this Journal (xxii, 82, 1881). The same author has recently further investigated the other minerals of the locality. The *columbite* had a red color in thin splinters. H.=5·5. G.=6·48. Analysis gave:

Ta_2O_5	Cb_2O_5	SnO_2	FeO	MnO	CaO	MgO	Y_2O_3(?)
84·81			5·07	8·05	1·27	0·20	0·82=100·22

The author remarks upon the high specific gravity, the unusually large percentage of MnO as probably accounting for the red color, and calculates the ratio of Ta_2O_5: Cb_2O_5 to be nearly 1 : 1; he, however, overlooks the fact that a *translucent* columbite, with G.=6·59, has been described from Branchville, Ct., which contained only MnO (15·58 p. c., 0·43 FeO) and had closely the ratio of 1 : 1 for the metallic acids (G. J. Brush and E. S. Dana, this Journal, xvi, 34, and Comstock, xix, 131).

Orthite occurs in imperfect blade-like crystals several inches in length. An analysis gave: SiO_2 32·35, Al_2O_3 16·42, Fe_2O_3 4·49, Ce_2O_3 11·14, La_2O_3 3·47, Di_2O_3 6·91, FeO 10·48, MnO 1·12, CaO 11·47, K_2O, Na_2O 0·46, H_2O 2·31=100·62. *Monazite* also occurs at the same locality in masses of large size (up to 20 pounds) as previously shown by König (Proc. Ac. Nat. Soc. Phil., Jan. 24, 1882). The analysis by Dunnington shows the presence of 18·6 p. c. ThO_2 and 2·7 SiO_2, and he remarks that these constituents may be present in the form of orangite; excluding them, the mineral is a normal phosphate of didymium, cerium and lanthanum.—*Amer. Chem. Journ.*, iv, 138.

11. *On the propagation of heat as determined by the crystalline structure of minerals and by the schistose structure of rocks.*—M. Jannettaz has undertaken a series of experiments in the line so successfully followed by Senarmont, and has obtained some new and interesting results. In regard to the propagation of heat in crystalline minerals, he finds, for example, that this depends upon the cleavage, taking place less readily in the direction of a normal to the plane of cleavage, than in a direction parallel to it. This distinction, however, does not apply to pseudo-cleavages, or in other words, to the lamellar structure resembling cleavage observed in some minerals, for example, sahlite. The author finds also that the relative orientation of the major and minor axes of the isothermal ellipsoid is characteristic for each group of minerals, *e. g.* the amphiboles, and differentiates this group from others. In experimenting with various rocks, M. Jannettaz has shown that true stratification is without influence on the propagation of heat, but that in schistose rocks (when the schistosity is due to lateral pressure), it takes place more readily in a direction parallel than in one perpendicular to the plane of schistosity.

12. *Milwaukee Clays and Bricks.*—The following are analyses by Mr. E. T. Sweet of (1) the Milwaukee clay that produces the buff-colored brick; (2) of a clay from Madison, Wisconsin, which burns red; and (3), of the Milwaukee brick.

	1.	2.	3.
Silica	38·22	75·80	53·78
Alumina	9·75	11·07	13·21
Peroxide of iron	2·84	3·53	4·92
Protoxide of iron	1·16	·31	·26
Carbonate of lime	23·20	2·45	
Carbonate of magnesia	15·83	·17	MgO 7·41
Lime (CaO)	3·24	·39	17·71
Potash	2·16	1·74	1·54
Soda	·65	1·40	·92
Water in composition	1·85	2·16	
Moisture	·95	1·54	·19
	99·85	99·56	99·94

Mr. Sweet suggests that the ingredients of the clay enter into a combination somewhat analogous to some members of the amphibole group.

12. *Sammlung von Mikrophotographien zur Veranschaulichung der mikroskopischen Structur von Mineralien und Gesteine* ausgewählt von E. Cohen, aufgenommen von J. Grimm in Offenburg. Stuttgart, 1881–82 (E. Schweizerbart'sche Verlagshandlung).—This work consists of a series of quarto plates illustrating the various points of interest in connection with the microscopic structure of minerals and rocks. Five parts have thus far been published, including forty plates, each giving four distinct photographs. The plates already issued cover a wide range of subjects, including the inclosures in minerals, as crystallites, microlites, glass inclosures, inclosures of fluids and gases; showing also their arrangement in crystals; cleavage; all the varieties of structure, fibrous, zonal, contretionary, etc.; the figures produced by etchings, by a blow or by pressure; also various optical phenomena, anomalous double-refraction and so on. The execution of the plates is worthy of the highest praise. Dr. Cohen deserves the thanks of all interested in studying or teaching this branch of mineralogy and lithology, for editing this invaluable work.

13. *Occurrence of Vivianite in Los Angeles County, California.*—Mr. Henry G. Hanks states that vivianite occurs with asphaltum at the Brea Ranch, Los Angeles County, California. The mass examined was a dark-colored earthy mineral, with streaks and veins of asphaltic substance, the whole being evidently the sandy desert soil blown over and cemented by the formerly liquid asphaltum. The vivianite is in small inclosed nodules, never larger than a pea and generally smaller. The mineral is that variety known as blue iron earth or native Prussian blue. It is soft, pulverulent; under the microscope it is crypto-crystalline.

14. *Helvite from Virginia.*—The rare mineral helvite has been identified by Professor H. C. Lewis among some minerals brought from the mica mine near Amelia Court House, Amelia County, Virginia. This is the locality which has furnished the large specimens of microlite (xxii, 82), and also monazite, orthite, etc. (see 10 above). The helvite occurs in crystals and friable crystalline masses imbedded in orthoclase, and generally associated with pale red topazolite. Its characters are: hardness about 6, specific gravity 4·306, color sulphur yellow, luster somewhat resinous, translucent. An analysis by R. Haines afforded (1); after the deduction of the gangue and the calculation of a portion of the MnO as Mn combined as MnS, the results in (2) are obtained:

	SiO_2	BeO	MnO	Al_2O_3	Fe_2O_3	CaO	K_2O	Na_2O	S	gangue	
(1)	23·10	11·47	45·38	2·68	2·05	0·64	0·39	0·92	4·50	9·22	=100·35
(2)	25·48	12·63	39·07	2·95	2·26	0·71	0·43	1·01	4·96	Mn 8·66	= 98·16

The author remarks that the last analysis does not correspond with the original helvite, which has 33 per cent SiO_2, so that a further investigation is to be desired.—*Proc. Ac. Nat. Sci. Philad.*, 1882, 100.

15. *Nickel ore in Oregon.*—It is announced that a deposit of a hydrated silicate of nickel and magnesia has been discovered on

Piney Mountain, Cow Creek, Douglas County, in southern Oregon. The mineral is amorphous, has an apple-green color; two varieties analyzed afforded 24 and 30 per cent of nickel oxide respectively, and 48 and 40 per cent of silica. In mineralogical character and in mode of occurrence the nickel silicate is closely related to the garnierite (and noumeite) of New Caledonia.

16. *Corundum from Lehigh County, Penn.*—The occurrence of large crystals of corundum near Shimersville, Lehigh County, Penn., is described by Edgar F. Smith and N. Wiley Thomas. The crystals have been found loose in the soil, thrown out in plowing. The largest crystal had a length of eight inches and a diameter of four and a half inches; another crystal, in the form of a double pyramid, had a length of five and a half inches and weighed over five pounds. A considerable number of smaller well-defined crystals have also been obtained.—*Am. Phil. Soc. Philad.*, March 17, 1882.

III. Botany and Zoology.

1. *Our Native Ferns and their Allies;* by Lucien M. Underwood, Ph.D., Professor of Geology and Botany in the Illinois Wesleyan University. Bloomington, Ill., 1882. 12mo. 134 pp. —This is a second and enlarged edition of a former work by the same author. "Our Native Ferns and how to Study them" included ferns only: the present volume has the remaining orders of fern-like plants. The work has nine chapters devoted to the habits, structure, physiology, description, literature and study of the plants, taking up nearly half the volume; and then follows the systematic and descriptive part, concluding with a well-ordered index and glossary. The work contains about thirty-seven illustrative figures, many of them from the author's own pencil. The subject is well-arranged, and concisely but sufficiently developed and explained, in the nine chapters, which taken together form a very useful introduction to the study of this class of plants. The genera and species are mostly arranged in accordance with the authorities which the author refers to; as well as most of the carefully worded descriptions and the statements of the geographical ranges. The species of ferns, including Ophioglosscæ, are 156, of the other orders, 55, making 211 recognized species of Pteridophyta, or Vascular Acrogens.

The price of the work is so low that no one need longer refrain from the study of these interesting plants on account of the cost of the needed literature. D. C. EATON.

New Haven, July 10, 1882.

2. *Europas och Nord Amerikas Huitmossor*; i. e. European and North American Peat-mosses (*Sphagna*); by S. O. Lindberg. Helsingfors. 1882. Pp. 88 and xxxviii, quarto.—Following upon Dr. Braithwaite's monograph of *Sphagnum*, comes this now from Professor Lindberg, with a copious morphological introduction. The twenty-one species are arranged under the three sections, *Eusphagnum* (which includes the groups, *Palustria*,

Subsecunda, *Compacta* and *Cuspidata*), *Isocladus* (for *S. macrophyllum*), and *Hemitheca* for *S. Pylaiei* and *S. cyclophyllum*); the ample characters are in Latin, and the synonymy is very full, —that of *S. cuspidatum*, for example, fills four pages. There is a complete index to the synonymous names. Although arranged primarily for Scandinavian use, the treatise will be welcomed by all bryologists. A. G.

3. *Nomenclator Zoologicus*, by SAMUEL H. SCUDDER. Part I. Supplemental list. 376 pp. 8vo. 1882. Bulletin of the U. S. Nat. Mus. Dept. of the Interior. Published under the direction of the Smithsonian Institution.—Mr. Scudder, in Part I of his Nomenclator, has given to zoologists, as the result of an immense amount of personal labor and assistance from many zoologists, a supplement to Agassiz's Nomenclator, making it to include all generic names employed in Zoology and Paleontology to the close of the year 1879, which are not contained or not correctly given in the works of Agassiz and Marschall, or in the Zoological Record Indexes. It contains 15,939 entries of genera, and besides these the genera of the Zoological Record for 1878 and 1879, and those of the Zoologischer Jahresbericht for 1879. References to places of first publication and derivations are also given.

The preface states that Part II, or the "Universal Index," contains about 80,000 references and includes the generic names in all previous lists. It gives the name of the genus (including also, in italics, such family or higher names as appear in Agassiz's Nomenclator or the author's Supplemental List); 2d. The authority; 3d. The group; 4th. The date; and 5th, the reference to the Nomenclators. It is also announced that *Decennial Supplements* will hereafter be issued by the Smithsonian Institution, the first to include the additions for the years 1880–1889. The Smithsonian Institution by these publications is doing great service to natural science throughout the world.

4. *Synopsis of the Fresh-water Rhizopods.* — A condensed account of the genera and species, founded upon Professor Joseph Leidy's "Fresh-water Rhizopods of North America." Compiled by ROMYN HITCHCOCK, Pres. N. Y. Microscopical Society. 58 pp. 8vo, with four plates. 1881. New York (Romyn Hitchcock). This is a convenient hand-book for all who would take up the examination or study of fresh-water Rhizopods, especially if they have not already the large and copiously illustrated work on the subject by Professor Leidy. This bottom branch of zoology is a fertile field for the capable embryological investigator.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *American Association.*—The circular of the Local Committee of the meeting at Montreal of August 23, states the following with regard to reduced rates of fare on railroads and steamboats.

The Grand Trunk, the Great Western, the Intercolonial, the

Quebec, Montreal, Ottawa and Occidental, the South Eastern and the Canada Pacific Railroads, the Richelieu and Ontario, the Ottawa and the St. Lawrence (Steamboat) Navigation Companies, will give tickets at half-rate for a single journey or at a single fare for going and returning over their lines. The steamboats of the latter company make daily trips down the St. Lawrence to Montreal, shooting the rapids, and connect at Clayton, above the Thousand Islands, with night-trains by the New York Central R. R. from Niagara Falls.

The Delaware and Hudson and the Central Vermont Railroads give tickets over their lines to go and return, at a single fare, the tickets on the two lines being interchangeable between Montreal and New York city. Negotiations are pending which, it is hoped, will secure similar arrangements with other railroad lines to the south and west. Those members whom this matter may concern will please address inquiries to one of the Honorary Secretaries at Montreal. Tickets at the above-mentioned rates, good for going and returning from August 10th to September 10th, will be furnished to persons presenting a certificate of membership of the Association for 1882. The following list of some principal points on the above-mentioned railroad lines in the United States may serve as a guide to members: Albany, Binghamton, Boston, Chicago, Detroit, New York, Portland, Providence, Saratoga, Springfield, Troy, Worcester.

There will be excursions to Ottawa and Quebec by the Quebec, Montreal, Ottawa and Occidental R. R.; to Lake Memphremagog by the South Eastern R.R.; and also one to Lachine by the Grand Trunk R. R.; which will be given by these lines free of charge.

Through the liberality of the Western Union and the Great North Western Telegraph Companies, telegraphic messages relating to family, social or scientific matters, will be sent from Montreal during the meeting, for members of the Association, free of charge, to all parts of the United States and Canada.

The Canadian, the National, and the United States and Canada Express Companies will undertake to forward and deliver promptly, free of charge, all parcels of books, drawings, instruments, or specimens of natural history for the use of members of the Association at the Montreal meeting, and will return the same at the lowest rates. The parcels should be addressed to the "*Care of Professor Bovey, A. A. A. S., McGill College, Montreal*," and should have their contents marked on the outside. By the courtesy of the Collector of Customs such objects will be admitted from the United States free of duty.

Letters to members after August 15th should be addressed A. A. A. S., Montreal, Canada. All enquiries respecting lodgings should be addressed to Mr. J. Bemrose, Secretary Nat. Hist. Soc., Montreal. Rates per day at Hotels: Richelieu, American and Albion, $1.50; St. James and Canada, $1.50 to $2.00; St. Lawrence Hall, $2.50 to $3.50; Windsor, $3.50 to $5.00.

The sessions, beginning August 23, at 10 A. M., will be held in the buildings of McGill University.

2. *Darwin Memorial Fund.*—The following persons have consented to act with the English Executive Committee in contributing and collecting funds for the Darwin Memorial: Asa Gray, Chairman; Spencer F. Baird, James D. Dana, Charles W. Eliot, D. C. Gilman, James Hall, Joseph LeConte, Joseph Leidy, O. C. Marsh, S. Weir Mitchell, Simon Newcomb, Charles Eliot Norton, Francis A. Walker, Theodore D. Woolsey, and Alexander Agassiz, Treasurer.

Subscriptions should be sent to Alexander Agassiz, Cambridge, Mass.

The Athenæum of July 8th states that the Darwin Memorial fund, which had already amounted in England to very nearly £2500, will take the form of a marble statue, and that the Trustees of the British Museum will be asked to place the statue in the large hall of the Museum (Natural History), at South Kensington.

3. *Transactions of the Connecticut Academy of Arts and Sciences.*—Volume iv, pt. 2, contains: Some interesting new Diptera by S. W. Williston; on the species of Pinnixa inhabiting the New England coast with remarks on their early stages, by S. I. Smith; occasional occurrence of tropical and sub-tropical Decapod Crustaceans on the coast of New England, by S. I. Smith; on the Amphipodus genera, Cerapus, Unciola and Lepidactylis described by Thomas Say, by S. I. Smith, with plate 2*a*; New England Annelida, pt. 1, historical sketch with annotated lists of the species hitherto recorded, by A. E. Verrill, with plates 3–12; the North American species of Conops, by S. W. Williston. Vol. v, pt. 2, consists of a paper by A. E. Verrill, on the Cephalopods of the north-eastern coast of America, with plates 26–41, 45–56; and Catalogue of the Marine Mollusca added to the Fauna of the New England region during the past ten years, by the same, with plates 42–44, 57, 58.

OBITUARY.

Dr. George W. Hawes, Curator of the Geological Department of the National Museum at Washington, died on the twenty-second of June last, at Manitou Springs, Colorada. Dr. Hawes was born December 31, 1848, at Marion, Ind. His parents died when he was very young and his early life was spent at Worcester, Mass. In 1865 he entered the Sheffield Scientific School in New Haven, and remained there until the end of his Junior year (1867) when he left to go into business in Boston. His natural taste for scientific pursuits, however, was too strong to allow of his being satisfied with such a life, and after trying this for four years, he returned to New Haven and was graduated at the Scientific School with the class of 1872. During the college year of 1872–73 he was private assistant to Professor Johnson in his chemical laboratory, and from 1873 to 1878 he filled with marked success the position of assistant and instructor in mineralogy and blowpipe analysis, in the Scientific School. In March 1878 he went

abroad and studied through the summer semester at Breslau with Professor A. von Lasaulx, devoting himself especially to microscopic lithology. On returning to New Haven he resumed his former position as instructor, and retained it until the following spring, when he again went abroad for further study. From March 1879 to June 1880, he was a student of mineralogy and crystallography at Bonn with Professor vom Rath, and of lithology at Heidelberg with Professor Rosenbusch. At the completion of his studies in Heidelberg he took there the degree of Doctor of Philosophy. He returned once more to his old place and duties in New Haven. But at the end of the year (1880) he accepted the position of Director of the Geological Department of the National Museum at Washington, which he held up to the time of his death. During the first half of the year of 1881, Mr. Hawes, besides carrying on his labors in connection with the Museum, was engaged on a special study of the building stones of the country, undertaken under the auspices of the U. S. Census. His interest in this work led him to overtax his strength, and before the close of the year it became evident to his friends that consumption had taken a strong hold of him; and in some nine months after the disease was first distinctly recognized, it had done its work.

The death of Dr. Hawes takes from the ranks of the younger scientific workers in the country one of the most gifted and promising. By years of patient study, urged forward by his own love of science, not by influences from outside, he had fitted himself to do the very best work in the subjects to which he had devoted himself. It is occasion for the deepest regret that so useful a life should be cut off thus prematurely. During the years of his residence in New Haven, Dr. Hawes published some twenty memoirs on various subjects connected with mineralogy and lithology. In 1878 he wrote a report on the mineralogy and lithology of New Hampshire, published as Part iv of the Geology of New Hampshire. This report covered 251 pages (4to) with twelve plates, and forms his most important contribution to science; it contains the results of extensive field work, as well as the microscopic examination of several hundred thin sections of rocks. It is throughout an excellent work and shows the careful painstaking way in which he carried out all that he undertook. Dr. Hawes's investigation of the building stones of the country, already alluded to, was a work in which he felt great interest, and which promised to yield most valuable results; unfortunately he was not allowed to carry it to completion.

Of the private character of Dr. Hawes this is hardly the place to speak. Though possessed of but few near family relatives, he had a singular power of winning personal friends, so that he leaves a wide circle to mourn his death. His purity and modesty of character, earnestness and uprightness of purpose, and unselfish interest in others will be long remembered by those who knew him well.